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TABLE OF CONTENTS

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THE LOGIC OF THE NORMAL LAW OF ERROR IN MENTAL MEASUREMENT

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No amount of practically successful "mental measurement" in laboratories, school-systems, factories or the army can relieve us, if we do not wish to waste time, of the necessity of stopping, every so often, to take account of first principles.

Psychophysics, with more than half a century of history to its credit, has repeatedly found the need to eliminate its logically unfit and reorganize its forces: it is a long cry from the principle of the just noticeable difference to the principle of the psychometric function. The mental test as a newcomer had first to prove its right to consideration. Now that it has been accepted it must pass under the critical eye and learn to conform. And what needs to be said, in way of admonition, applies especially to the mental test, although for no other reason than that the mental test is the lustiest form of mental measurement that one meets today. It, especially, merits a discriminating encouragement.

1. THE NATURE OF PROBABILITY

We can get nowhere in an understanding of mental measurement unless we appreciate the relation of the unit of measurement to the frequency of occurrence of the different measures. In statistics we deal repeatedly with frequency-distributions; and we know, very often, the frequencies before we are assured as to what they are frequencies of. It is not unusual to attempt to calibrate a scale of measurement in terms of frequencies,—an attempt that always demonstrates the

necessity for a definite understanding of the relation between the two. Frequency-distributions, however, have a way of absorbing some of the mystic power that is commonly supposed to inhere in the normal law of error, the 'probability-curve,' which is itself a frequency-distribution. Into the mystery of this function, a mystery connected with its supposed a priori nature, we can pry only by studying its origins, logical and historical. On the logical side we must ask what, in scientific usage, is the nature of a probability. In such a beginning we are setting ourselves no mean task, but a necessary one if we are to avoid muddling.

The early history of the theory of probabilities is a history of the solution of problems arising in games of chance.[1] An applied mathematics of probability preceded the pure. For us the most significant principle that this period brought forth is one that is implicit in the famous theorem of James Bernoulli (1713).

This theorem holds that events tend to occur with relative frequencies proportional to their probabilities. If *a* is twice as likely as *b* to happen, then in the long run it will happen twice as often. But how, we may well ask, are we to know the probabilities of occurrence for *a* and *b*? We must exclude the inverse use of the Bernoullian theorem. We can not say that *a* is twice as likely to happen as *b* because it happens twice as often, for this is simply to reason from frequency to probability and ultimately to identify the two. Our problem was to predict frequency from given probability. How then do we achieve this given probability?

The case of the coin appears at first glance fairly obvious.

> If we may regard an ideal coin as a uniform, homogeneous circular disc, there is nothing which can tend to make it fall more often on the one side than on the other; we may expect, therefore, that in any long run of throws the coin will fall with either face uppermost an approximately equal number of times, or with, say, heads uppermost approximately half the times.[2]

Thus we read in a modern textbook of statistics. The thesis is: the probabilities are equal when there is no good reason for their being unequal. Von Kries has called this law the principle of insufficient reason ("*das Princip des man-*

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[1] I. Todhunter, *History of the mathematical theory of probability*, 1865, is the standard historical work. He begins with Pascal and Fermat in 1654 and continues to Laplace: 624 pp.

[2] G. U. Yule, *Introduction to the theory of statistics*, 4th ed., 1917, 256.

gelnden Grundes"); [3] Boole speaks of it as "the equal distribution of ignorance." [4]

Against an indiscriminate use of the principle of insufficient reason von Kries inveighs. We are asked to consider before the days of spectroscopic analysis the question of whether there is iron in Sirius. We are completely ignorant, but we can conceive of no greater reason for the presence of iron than against it. The probability for the existence of iron in Sirius is therefore ½, on the principle of insufficient reason. Similarly the probability for the existence of gold is ½. It follows algebraically that the probability for the existence of both iron and gold in Sirius is ¼, of iron without gold ¼, of gold without iron ¼, and of neither of these metals ¼. There were, when von Kries wrote, 68 known earthly elements. The probability of the existence of all these elements in Sirius is the 68th power of ½; *i. e.* the chance of all the elements existing in Sirius, as well as the chance of no one of them existing there, is about three in one-hundred-billion-billions. But if we had raised in the first place the question of the existence of earthly elements in Sirius, we might, being ignorant and applying the principle of insufficient reason, have said that it is just as likely that there are no earthly elements in Sirius as that there are some. The discrepancy between the three one-hundred-billion-billionths and one-half leads von Kries to declare that "die Aufstellung der gleich möglichen Fälle muss eine in zwingender Weise und ohne jede Willkür sich ergebende sein." [5]

Plainly it must. Shutting our eyes reduces our faith in what we see to less than one-half. If we would know the nature of our surroundings, we must open our eyes; and, if we would acquire a tenable belief of the chemical constitution of Sirius, we must await the spectroscope. There is no alchemy of probabilities that will change ignorance into knowledge. Expectations must be founded upon cogent rather than insufficient reasons. Nevertheless we can not, it would seem, push this antithesis too far. We can never be completely informed of all the conditions. Thus Westergaard: [6]

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[3] Joh. von Kries, *Die Principien der Wahrscheinlichkeitsrechnung*, 1886, 6. The book is "eine logische Untersuchung." See also A. Kaufmann, *Theorie und Methoden der Statistik*, 1913, 43ff. C. Stumpf, in *Sitzungsber, d. k. bayr. Akad. zu München* (*philos.-philol. Cl.*), 1892, 37-120, in arguing against von Kries, appears to defend the principle of insufficient reason; but in so doing he appeals to a wider analysis of the situation that is not wholly unlike a principle of cogent reason. See especially 41, 61-79.

[4] G. Boole, *An investigation of the laws of thought*, 1854, 370.

[5] *Op. cit.*, 10f.

[6] H. Westergaard, *Grundzüge der Theorie der Statistik*, 1890, 4.

In every well-arranged game of chance . . . the balls are of the same size, of the same wood, of the same specific gravity, *etc.* They are mixed with the greatest care and every ball is apparently subject to the same forces. Yet such is not the case. In spite of all effort the balls are different, and depart, even though quite insignificantly, from spherical form. One approaches this and another that mathematical form. Every one has a weight and size different from all the others. No ball is absolutely like any of the others. Moreover they can not possibly lie in the bag in exactly the same manner. In short there are a multitude of apparently insignificant differences which determine that exactly this ball and none other shall be drawn.

In the face of such complexity it is hopeless to seek cogent reason for the drawing of the particular ball.

Moreoever, there is a logical as well as a practical reason for our inability to predict in the individual case. Westergaard seems to see this when he implies—it amounts to this—duplicity on our part when we arrange the game of chance. We attempt to make the balls all equal so as to equalize chance, we assume that we have succeeded for all practical intents and purposes, and yet in the same breath we expect to be able to draw some particular one of the balls. However, there must be inequalities, including of course difference of spatial position within the bag, in order to make the drawing possible.

Did no such inequalities exist, then one of two things would happen: either all the balls would turn up at once or they would all remain in the bag.[7]

In more general terms, we may say that the problem of probability exists only in the face of ignorance. Given all the conditions, we deal not in probabilities but with the certainties of cause and effect.[8]

Here then it would seem we are landed in a dilemma. If we are in total ignorance of the conditions of our problem, all argument is foolishness. If we know all the conditions, we may argue to an effect but not to a probability. Have we to deal then either with a practical impossibility or with a logical absurdity?

The solution lies in the adoption of a definite intermediate position. Fisher lays down a first principle in the case:

It thus appears that a rigorous application of the principle of cogent reason seems impossible. However, a compromise between this principle and that of the principle of insufficient reason may be effected by the following definition of equally possible cases, viz.: *Equally possible cases are such cases in which we, after an exhaustive analysis of the physical laws underlying the structure of the complex of causes*

[7] *Loc. cit.*

[8] Thus J. Venn, *Logic of chance,* 3rd ed., 1888, discusses probability as "amount of belief," where "ignorance of the individual is presupposed" and "causation . . . denied, within considerable limits:" p. 122.

influencing the special event, are led to assume that no particular case will occur in preference to any other.[9]

But Fisher, in attempting a compromise, neglects to tell us definitely how much ignorance and how much knowledge make the proper conditions from which we may proceed.[10] We are scientists engaged in very practical work and we wish to know what we may and what we may not do in our statistics.[11]

We can get at the nature of probability by taking a simple instance. The event in which we are interested is, let us say, the toss of a penny. We wish to know the probability that it will come heads. The law of insufficient reason says that, since we can not know that the penny is more likely or less likely to come heads than tails, the probability of heads is ½. The extreme demand for cogent reason would urge that, since we can not know the principal determining factors, this case is insoluble. The 'compromise' that Fisher proposes is that we shall analyze the underlying physical laws. We shall find that one side of the penny is heads and the other tails, that the penny is homogeneous, that it spins many times in the toss, and so on, until we are "led to assume" that neither heads nor tails will be preferred. Now the crux of the whole matter lies in the mechanism by which we are led to this assumption. In the case of the penny no knowledge of physical constitution and of the complexity of tossing would ever be valid ground, in itself, for believing that the penny would come heads as often as tails. In the initial instance this fact

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[9] A. Fisher, *Mathematical theory of probabilities*, 1917, vol. I, 9. An interesting book, written with historical and philosophical perspective for practical purposes. It emphasizes the work of Continental statisticians in a way that English writers do not. Volume II seems not yet to be published.

The position is approximately Kaufmann's, *loc. cit.*, in distinguishing between *mangelnder* and *zwingender Grund.*

[10] It will not keep the statistician from error to tell him that his analysis must be "exhaustive" or that it must be so exhaustive as to 'lead him to assume . . .' Not only do investigators differ in natural thoroughness, but the problem may be restated, as we shall see, so that analysis of the properly known conditions may be pushed to the limits of cogency.

[11] The present paper in its inception is an attempt to answer certain questions that arose in the Office of the Surgeon General of the U. S. Army with respect to the ratings of recruits on intelligence tests. Actually logic is not a far cry from practice, but the technologist upon occasion can be deaf.

must be observed.[12] Thereafter we may reason that other homogeneous, one-headed, well-tossed pennies will in the long run, come heads fifty per cent of the time, and we may even go farther and reason that a homogeneous, six-faced, well-tossed die will in the long run come up equally often on every face. What we do is to assume probability on the grounds of observed frequency and its conditions.

Let us state the case in roughly syllogistic form.

The tossing of a penny gives, when indefinitely repeated, 50% heads;
The event in which we are interested is a tossing of a penny;
The event in which we are interested gives, when indefinitely repeated, 50% heads.

This leads us to predict what will happen to our particular penny, if it is tossed repeatedly; and, what is more important, it defines for us the place of cogent reason and the sphere of ignorance. We must know what a penny does when tossed again and again, either directly from observation or indirectly by inference from other observations. The major premise must be based upon cogent reason and there is no limit to the exactitude of the knowledge. Ideally the major premise may be considered to be indubitably established. Moreover we must know that the event in which we are interested is the toss of a penny in the sense of that term in the major premise. Our minor premise, by "an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the special event," must also be established and without limit of accuracy by cogent reason. Of what then do we remain ignorant? Of the manner in which the particular, in the middle term, is subsumed under the general. In other words we know that our event is such an event that if repeated it will yield in the long run heads and tails equally, but we do not know whether this particular event, unrepeated, will be heads or tails.

Of an equal distribution of this ignorance, of a law of insufficient reason, we are saying nothing. But have we attained to a statement of probability by saying of a single event that

[12] "The assumption that any probability-constant about which we know nothing in particular is as likely to have one value as another is grounded upon the rough but solid experience that such constants do, as a matter of fact, as often have one value as another:" F. Y. Edgeworth, *Mind* O. S. 9, 1884, 230. K. Pearson approves this view: *Grammar of science*, 3rd ed., 1911, pt. i, 146. See also Edgeworth in article on Probability, *Ency. Brittanica*, 1911, xxii, 377, 391. There have been many attempts to supply "the rough but solid experience" empirically; *e. g.* Edgeworth, *Cam. Philos. Trans.* 20, 1905, 128ff.; Pearson, *Chances of death*, 1897, i, 13ff., 48; Westergaard, *op. cit.*, 21-56.

it is such that, if repeated, it would yield a given frequency of particular cases?

We can not within the narrow limits of this paper escape a certain amount of dogmatism. We must read widely elsewhere on the logic of chance. This much may, however, be said. It is plain that the probability of the event can not inhere in the event. The coming throw is either heads or tails, not half heads and half tails. An event is not even conditioned upon its probability. If, from a box of one thousand tickets, numbered consecutively, one draws any ticket at all, one obtains that particular ticket in the face of odds of 999 to one against it. The rule of such a drawing is that the improbable always happens. If the probability does not lie in the event, it must lie outside it; and we may here assert that it lies in the series to which the event belongs. When we ascribe a probability to a particular event we are simply seeing that event in a series in which the event is repeated as it varies in some particular phase; or, to put it more picturesquely, we see a series of repeated events telescoped within the single instance. This is our habitual mode of dealing with ignorance of this kind. Unable to prophesy the particular event, but able to prophesy its frequency, we content ourselves with the general in view of our impotence to deal with the particular.[13]

(And we may note in passing that ignorance has not bred knowledge in this process. Where reason was insufficient, prediction failed. We never can tell what will turn up next. We may be able to tell what will happen one way often and another way seldom, but that tells us not at all what the next one is).

[13] The paragraph is compatible with Venn's chapter on *Measurement of belief, op. cit.*, 119ff., *q. v.*, although divergent in emphasis. See also the discussion by F. M. Urban, Ueber den Begriff der matematischen Wahrscheinlichkeit, *Vierteljahrschrift f. wiss. Philos. u. Soziol.* 35, 1911, 1-49, 145-185. Only the margins of the logical problem are skirted here. Much might be said of subjective and objective probability. For the scientist-psychologist it is perhaps useful to distinguish four kinds of probability: (1) observed frequencies; (2) expected frequencies; (3) participation in a series of expected frequencies; and (4) psychological expectation. (1) is scientific fact; (2), derived from (1), is a prediction; (3) is a probability, a 'telescoped' (2); (4), the actually observable mental expectation, is a function of (3) and other conditions for determining tendencies, which the psychologist might do well to investigate. See Venn, 152-162. The metaphysical problem of the relation of physical constitution to frequency is beyond the point; actually the relation is observed. In more exact usage it would be necessary to assume that the phrase, 'indefinite repetition,' be taken to mean that the given frequency is approached as a limit when the repetitions are multiplied. The approach would not be continuous; Venn, 118.

We may seem far from the subject of mental measurement, but not without occasion, as we shall see. Let us turn now to the part played by the normal curve of error.

2. The Role of the Law of Error

The normal law of error[14] has been both an inspiration and a limitation in statistical measurement. Its formulation by Laplace resulted during the last century in a wide extension and application of the law to numerous forms of scientific, social and biological measurement; but Laplace is not responsible for all that occurred. There is a bit of magic in the formula. The law came to play the part of a first principle of nature, of an ideal, given *a priori*, to which nature seeks to conform. The mathematicians wrought slowly, but they wrought a god. Against such blind faith later statisticians have protested. They call the normal law a "fetish" and its *a priori* use a "superstition." Nevertheless the "superstition" still lingers and is mixed up with mental measurement. For this reason we are going to enquire, concerning the law of error, what real value it has for us to-day as a scientific tool.

In the days when the application of the theory of probabilities was largely confined to games of chance,[15] the logical difficulties discussed above had little opportunity to make trouble. The fact is that the cards in a pack, the faces of a die, and the sides of a coin do in the long run and under the conditions of gaming turn up about equally often. At this level common sense takes care of the theorist. It makes no practical difference whether the reason he assigns for the equality of occurrence is his lack of sufficient reason for any other result or the empirical fact that these things do work in such a way. Nor does it matter if unwittingly he applies to the single event the frequency that belongs to the series and calls it the probability of the event, for he knows by common sense that probability is not prophecy and that he

[14] The so-called "Gaussian" curve. The mathematical propadeutics for this function were prepared as long ago as the beginning of the 18th century (De Moivre, 1718). See Todhunter, *op. cit.* 71f., 136, 191ff., 552f. Laplace's *Theorie analytique des probabilités* brought together and supplemented in 1812 his work of thirty-five years, and gives the development of the law of error on pages 275ff. The proper date for the law is presumably 1786; see Todhunter, 485. Gauss gives the formula in Article 178 of the *Theoria motus corporum coelestium*, 1809, for which see his *Abhandlungen zur Methode der kleinsten Quadrate*, 1887, 102f. On his ascription of credit to Laplace, see *ibid.*, 207.

[15] The period covered by Todhunter, *op. cit.* Todhunter leaves off unfortunately where our most important period begins.

can not be assured of a realization of his expectations except in the long run. The fact is that the principle of insufficient reason and an indefinite conception of the nature of probability do when applied to games and modified by such unconscious reservations, furnish safe ground for practice, even though the logical implications are dubious.

Laplace and Gauss extended the theory of chance to the probability of errors.[16] Errors are a new material and it is not plain at once that the old assumptions hold. So far as the development of the law of error goes, these mathematicians were content to make their assumptions and reason abstractly to the general conclusion. Laplace worked in the tradition of the old school. He developed the law of error as the limiting case of the binomial expansion when the number of terms is infinite. This process is equivalent to assuming that, in a given case, an error is the resultant of a large number of sources of error, every one of which may affect the result in one way or the opposite (or affect it or not affect it), *i. e.* every source of error may, in the particular case, come up "heads" or "tails," as it were.[17] Either we have, in practice, to assume the principle of insufficient reason, which, applied to errors, does not have the sanction of common-sense experience; or we have to achieve an analysis of the case into sources of error and determine for every one that its law is the law of the coin, a course which is ordinarily impossible. Gauss hypothesizes explicitly that positive and negative errors are equally likely, and derives the law on this basis. Logically his assumption is the same as Laplace's, but it is usually stated in a form that suggests a test. If positive and negative errors are equally likely, then the arithmetical mean is the most probable value. To test the validity of the assumption in practice, we have then only to select a series (of observations, say, which are subject to error) and see whether the arithmetical mean occurs most frequently.

Laplace's derivation suggests the nature of error, Gauss' its mode of manifestation. Both these famous mathematicians were arguing from premise to conclusion when they derived the law of error, and science remains in their debt. We have no complaint to make until we find the law applied without an effort to establish the premises in the particular case. *The*

[16] *Cf.* W. S. Jevons, *Principles of science,* 1883, 375-385.

[17] Jevons, *op. cit.,* 380f. L. D. Weld, *Theory of errors and least squares,* 1916, 41-56, has an elementary discussion of the binomial expansion and the law of error. For the logic of this derivation applied to psychophysics, see E. G. Boring, *Am. J. Psychol.* 28, 1917, 465ff.

law will not work for errors, unless errors fulfill its conditions.

Gauss was clear about his assumptions, but he did not hesitate to apply the law to astronomical errors of observation.[18] Some of his followers, it seems, who had not witnessed the birth of the law, were less concerned in its application with its parentage. In theory of measurement the law was often accepted on its face value, in spite of the fact that a constantly increasing list of exceptions to it was being made out. In such cases the argument for the law seems to have been the practical one that it gave but little trouble in actual use. Thus Urban recently wrote:[19]

> We are confronted by the shocking situation that a proposition is triumphantly borne out by an immense indirect experience and that it can be proved neither by mathematical deduction nor by direct experience.

Urban, however, did attempt to prove it by showing, under the conditions of the psychophysical experiment, that positive and negative errors of observation, measured from the point of subjective equality, are equal.[20] Faith in the principle was established, however, long before psychophysics furnished this justification.

It is upon Quetelet, it seems, that we must fix the responsibility for the uncritical extension of the normal law to various human measures other than errors.[21] In making a man, he holds, Nature aims at an ideal, and the differences

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[18] On the assumptions: Gauss, *op. cit.*, 4; on the applications: 92ff., 129, 139ff., *etc.*

[19] Urban, *Psychol. Rev.* 17; 1910, 242.

[20] *Op. cit.*, 240-244.

[21] An interest in the application of theory of probabilities to a variety of human problems was of long standing. Todhunter mentions the following initial attempts at application: expectation of life (Graunt, 1662), human testimony (Craig, 1699), human innocence (N. Bernoulli, 1709), birth-rates (Arbuthot, 1710-12), sex-ratios (De Moivre, 1718), astronomical observation (Simpson, 1757), marriages, smallpox, and inoculation (D. Bernoulli, 1760), weather forecasts (Lambert, 1773), annuities (Lagrange, 1798), modes of election (Laplace, 1812), and scientific observation in general (Laplace and Gauss). The important names are N. Bernoulli, De Moivre, D. Bernoulli, Lagrange, Trembley, and Laplace. See Todhunter, *op. cit.*, 37, 41, 54f., 193, 196, 211, 228f., 265, 320, 335, 349, 423, 426, 446, 500f., 546, 589-613, 615ff.

Laplace in the *Essai philosophique sur les probabilités*, which is a separately published introduction to the *Théorie* (*op. cit.*), devotes space to the application of probabilities to natural philosophy, to the moral sciences, to testimony, to elections in assemblies, to the judgments of tribunals, to tables of mortality, and to the duration of life, marriages, *etc.* Gauss is interested in applications to astronomy and geodesy. But all these things were more or less subject to a law of errors. Quetelet's conception of any natural variation as an error is

between men represent her degrees of error.[22] Quetelet refers for evidence to the measurement of circumferences of the chests of 5,738 Scotch soldiers and of the heights of 100,000 French conscripts. His position would be all very well were Quetelet content with supporting his hypothesis by this evidence. He seeks, however, to make it work both ways. He suggests that the deviation of the curve for the French conscripts from the normal law is due to fraudulent rejection for military service.

> This . . . law of continuity enables us to recognize a more remarkable fact: we might suspect it, but here we find it proved,—it is that the number of men rejected for deficiency in height is much exaggerated. Not only can we prove this, but we can determine the extent of the fraud. The official documents would make it appear that, of 100,000 men, 28,620 are of less height than 5 feet 2 inches: calculation gives only 26,345. Is it not a fair presumption, that the 2,275 men who constitute the difference of these numbers have been fraudulently rejected?[23]

Of course it is not a fair presumption, since the law depends on the official documents for confirmation. Nevertheless, while admitting the dependence of the law on experience, Quetelet proceeds in numerous cases to analyze experience by means of it. Such a double-edged sword is a peculiarly effective weapon, and it is no wonder that subsequent investigators were tempted to use it in spite of the necessary rules of scientific warfare.

Galton in the *Hereditary Genius* applied the normal law to

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an undoubted extension of the principle, although it is difficult to draw a line.

A. Quetelet was a Belgian statistician, who published an *Essai de physique sociale* in 1835 and a collection of *Lettres sur la théorie des probabilités* in 1846. He writes in the tradition of Laplace with little reference to Gauss. That Gauss' work on least squares was of interest to the French school is evidenced by its translation into French (from the Latin) in 1855, thirty-two years before a similar publication in the German language.

There is no question as to Quetelet's responsibility for the extension of the principle: *cf.*, *e. g.*, W. Lexis, *Zur Theorie der Massenerscheinungen in der menschlichen Gesellschaft*, 1877, 11; Kaufmann, *op. cit.*, 12, 163.

[22] *Letters on the theory of probabilities*, trans. 1849, 91-96. "The difference which Nature makes in the heights of men is not greater than that which inexperience would produce in the measurements taken on one individual man in an attitude more or less curved." "Everything occurs as if there existed a type of man from which all other men differed more or less." "Each people presents its mean, and the different variations from this mean, in numbers which may be calculated *a priori*." (pp. 95f.)

Cf. the discussion and criticism of "l'homme moyenne" in Kaufmann, *op. cit.*, 110ff.

[23] *Letters*, pp. 97f.

mental differences and, using it *a priori,* worked from frequencies of natural ability to a scale of equal intervals of ability. He frankly accepted "the very curious law of 'deviation from an average'" from Quetelet, showed that certain mental capacities obeyed the law approximately, and then argued from the law to "grades of natural ability, separated by equal intervals." [24]

The English school of biometricians, which derives directly from Galton, has furthered the *a priori* use of the normal law that obtains to-day in much biometric and mental measurement.[25] There is an early study by Pearson that illustrates the point.[26] Weldon had measured the lengths and breadths of two large samplings of crabs: one set from Naples and one from Plymouth. The Plymouth batch gave symmetrical distributions, but the frequency curve for the breadths of the Naples batch was skewed. Pearson, by an elaborate mathematical treatment, analysed the skew curve into the sum of two normal curves, the averages of which did not coincide, and Weldon accepted this result as evidence of "dimorphism." Pearson then applied his method to a symmetrical curve for the Plymouth crabs and found that there was no real solution. He assumed, therefore, that these data were "homogeneous." Further he laid down the general rule that some such analysis must be attempted upon every apparently normal curve before a conclusion can be reached as to the homogeneity of the data. If the actual results can be better represented by the sum of two normal curves than by any single normal curve, then there is heterogeneity. Even curves that approximate the normal very closely must be tested, for they may yield to analysis.

Thus Pearson showed that the distribution of shots at a

[24] F. Galton, *Hereditary Genius,* 1869, 26-36. G. Th. Fechner, of course, had already used the law in the method of right and wrong cases with sensory material. He seems not to have hesitated in his acceptance of the law from Gauss: *Elemente der Psychophysik,* i, 1859, 104. He applied it, not to sensations, but to the *scheinbare Reizgrösse,* the effective inner stimulus: *Revision der Hauptpuncte der Psychophysik,* 1882, 40f., 45. Current notions of "deviation from an average" come undoubtedly, however, from Galton and Quetelet.

[25] See, for example, M. R. Trabue, *Completion-test language scales,* 1916, 29-60, where the normal law is assumed in order to obtain a scale of equal units. Trabue is quite explicit about the assumption (see note, p. 31), but he evidently believes it is correct or he would not devote so much space to developing it.

[26] K. Pearson, Contributions to the mathematical theory of evolution, *Philos. Trans.* 185A, i, 1894, 71-110. The basal data are given by W. F. R. Weldon, *Proc. Roy. Soc.* 54, 1893, 318-329.

target, given by Merriman,[27] is best represented by the sum of two normal curves. The gun must be thought of as aiming, not at the sixth band of the target as Merriman supposed, but at the fifth and seventh bands (explainable perhaps by a change of sighting during mid-firing). Here then, we find no attempt at all to seek empirical support for the normal law. The crabs were obtained in a single selection and the shots were made in a single experiment, and each set appears, even after it is charted in a graph, as if all the measures belonged together. But we apply the criterion of the normal law and we find the crabs dimorphic and the gun double-sighted.[28]

Now the very remarkable thing about this easy acceptance of the law of error as the rule of nature is that it has been done all along in spite of very much better available information. Let us get some idea of what has been said.

We saw that Laplace's assumptions were implicit in his method and Gauss' explicit in his text. We had no quarrel with them, but we blamed Quetelet. Did Quetelet's thesis pass, then, quite unchallenged? No, for in the notes to the *Letters* he printed three letters from Bravais,[29] who presented figures on the skew distribution of barometric pressures, discussed various reasons for departures from the normal law, and said in brief:

> I think that generally every partial and distinct cause of error gives place to a curve of possibility of errors (or, if preferred, of differences about the mean), which may have any form whatever,—a curve which we may either be able or unable to discover, and which in the first case may be determined by consideration *a priori* on the peculiar nature of this cause, or may be determined *a posteriori* by observations on the isolated condition of other concomitant causes of error.[30]

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[27] M. Merriman, *Textbook of the method of least squares,* 1884, 14. These data have frequently been quoted as illustrating the application of the normal law to errors.

[28] Of course, if we wish to define "heterogeneity" solely by reference to the applicability of the normal law, nothing can be said, though the usefulness of the procedure might be questioned. The almost unavoidable implication of such a course, however, is exactly that which Weldon got from it, *viz.* that the two normal components point to a biologically significant dimorphism. That idea, moreover, originated with Quetelet: *Letters,* 96f. On the other hand, it is only fair to Pearson to point out that the theory of skew variation in homogeneous material must already have been taking shape; see his distinction in the year following between asymmetry due to heterogeneity and asymmetry due to the mode of variation of a homogeneous material: *Philos. Trans.* 1862, i, 1895, 344f.

[29] Quetelet, *Letters,* 286-295. Venn, *Nature,* 36, 1887, 411f., shows the same asymmetry of barometric heights that Bravais pointed out.

[30] P. 290.

In introducing the letters from Bravais, Quetelet remarked:

In this memoir I only had in view the examination of the case in which accidental causes have had no tendency to act in one direction more than another.[31]

We have already shown that Quetelet actually established the law by the data and then corrected the data by the law.

In 1869 Galton accepted the law of deviation from an average as applicable to mental ability, but in 1879 he was ready to insist on exceptions:[32]

My purpose is to show that the assumption which lies at the basis of the well known law of "Frequency of Error" . . . is incorrect in many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success and corresponding convenience. . . .

The assumption to which I refer is, that errors in excess or deficiency of the truth are equally probable; or conversely, that if two fallible measurements have been made of the same object, their arithmetical mean is more likely to be the true measurement than any other quantity that can be named. . . .

Suppose we endeavor to match a tint: Fechner's law, in its approximative and simplest form of sensation=log stimulus, tells us that a series of tints, in which the quantities of white scattered on a black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the eye to be separated by equal intervals of tint. Therefore, in matching a grey that contains 8 portions of white, we are just as likely to err by selecting one that has 16 portions as one that has 4 portions. In the first case there would be an error in excess, of 8; in the second there would be an error in deficiency, of 4. Therefore, an error of the same magnitude in excess or in deficiency is not equally probable.

Thus Galton argues that the geometric mean is the most probable value with sensory material or data that follow a similar law. The distribution curve in such cases would be markedly skew.

We remarked that Pearson in 1894 gave evidence of being influenced by the sanctity of the normal law. His faith was not great for he noted its limitations in 1895.[33] In 1900 the faith was gone.[34]

It appears to me that, if the earlier writers on probability had not proceeded so entirely from the mathematical standpoint, but had endeavored first to classify experience in deviations from the average, and then to obtain some measure of the actual goodness of fit provided by the normal curve, that curve would never have obtained its present position in the theory of errors. Even today there are those who regard it as a sort of fetish; and while admitting it to be at fault

[31] P. 286.

[32] *Proc. Roy. Soc.* 29, 1879, 365-367. The paper is followed by a discussion by D. McAlister, *ibid.*, 367-376 which gives the mathematics and presents some graphs of skew distributions that would result from the logarithmic scale of measurement.

[33] *Philos. Trans., loc. cit.*

[34] *Philos. Mag.*, 5th ser., 50, 1900, 173.

as a means of generally describing the distribution of variation of a quantity x from its mean, assert that there must be some unknown quantity z of which x is an unknown function, and that z really obeys the normal law! This might be reasonable if there were but a few exceptions to this normal law of error; but the difficulty is to find even the few variables which obey it, and these few are not usually cited as illustrations by the writers on the subject.[35]

Pearson in developing a system of skew curves whereby he seeks to adjust theory to nature has performed great scientific service in combatting the superstition of the normal law.[36] The paper of 1894 suffered on account of its occasion, *viz.*, the problem of dimorphism in Weldon's crabs. But not all investigators have avoided initial error; and presently we must return again to Pearson.

These quotations from Galton and Pearson suggest a reason why the normal curve should be of exceptional occurrence. If "log stimulus" gives a normal curve, says Galton, then "stimulus" will not. If x does not give a normal curve, some persons, says Pearson, assert that z, of which x is a function, does follow the law. Suppose then that nature does conform to the normal law; we shall not in practice be fortunate enough to obtain normal curves of distribution unless we have chanced to use nature's own unit of measurement, or some unit directly proportional to it. Bertrand pointed this out long ago.[37]

La règle des moyennes, il importe d'insister sur ce point, n'est ni démontrée ni exacte. S'il était admis que la moyenne entre plusieurs mesures fût toujours la valeur la plus probable, il en résulterait des contradictions. Quand on mesure une grandeur, on mesure, par cela même, toutes les fonctions de cete grandeur, son carré par exemple, ou le logarithme du nombre qui la représente. Pourquoi la valeur la plus probable du carré ne serait-elle pas la moyenne des valeurs obtenues pour le carré, et la valeur probable du logarithme, la moyenne des logarithmes? . . .

Il ne faut pas, pour écarter l'objection, faire une distinction entre les grandeurs directement mesurées et celles qui résultent d'un calcul. Un mécanicien pourrait, bien aisément, annexer à une balance une aiguille marquant le carré ou le logarithme du poids. Ce carré ou ce logarithme deviendrait alors la grandeur mesurée. Le postulatum admis dans un cas devient donc impossible dans les autres.

A better example would be the distribution in size of the cubical crystals of common salt. In a sample of such crystals we might measure size by the height of the cube or by its

[35] The remarks are occasioned by Merriman and the target-shooting. Merriman has subsumed these data under the "fetish"; but we have seen that it is not perfectly clear why Pearson in 1894 subsumed the same data by a two-fold application of the same "fetish."

[36] The early papers on "Skew variation in homogeneous material" are *Philos, Trans.* 186A, i, 1895, 343-414, and 197A, 1901, 443-459. See also "On the general theory of skew correlation and non-linear regression," *Drapers' Co. Research Memoir,* 1905.

[37] J. Bertrand, *Calcul des probabilités,* 1889, 180f.

weight. Since the latter measure should be approximately proportional to the cube of the former, it is plain that the normal law could not hold in both cases.[38] Now, if nature seeks to conform to the normal law, does she prefer to conform in lengths or in volumes or, perhaps, in some other more "natural" unit? At least, if man would seek the law of nature, he must have cogent reason for his choice of measures.[39] He can not (as the behaviorist would agree!) tell what nature is doing unless he knows what nature is trying to do.

We have been speaking of France and England, but the reaction against the "Gaussian dogma" was more explicit in Germany. In 1877 Lexis published a *Theorie der Massenerscheinungen*[40] in which frequency distributions were dealt with as normal, subnormal, or supernormal.[41] The emphasis was thus shifted from the normal distribution to normal deviations from the normal distribution.[42] The "Lexian ratio" measures this deviation.[43] This point of view has met with acceptance among German and Scandinavian actuarial statisticians, and forms, for example, the basis of Czuber's *Wahrscheinlichkeitsrechnung*.[44] We have already seen that von Kries in 1886 did not include among valid logical principles the law of insufficient reason. He was inclined to believe, however, that in many cases the "Gaussian law" was realized,[45] and he cites in support Bessel's observations, which Urban refers to as an example of asymmetry.[46]

Westergaard in 1890 sums up the empirical case.[47] He

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[38] It would be interesting to test this relation in the case of heights and weights of men. Height-and-weight tables are available in anthropometrical and somatological texts.

[39] We have a cogent reason, presumably, when, with the Fechner pendulum, we take as the unit, not the angle read on the scale, but the square of the sine of half that angle.

[40] Lexis, *op. cit.;* see especially 34ff.

[41] Fisher's translation of *unternormal* and *übernormal* as "subnormal" and "hypernormal" is due presumably to his otherwise excellent "dano-English," for which he so modestly apologizes: *op. cit.,* 125 and xv.

[42] If a pun is permissible, for Lexis' thesis is that the "normal curve" is not "normal" in the sense "usual."

[43] *V.* Fisher, *op. cit.,* 124ff.

[44] E. Czuber, *Wahrscheinlichkeitsrechnung,* 2 Aufl., 1910, 34-78.

[45] *Op. cit.,* 226ff.

[46] *Op. cit.,* 241. Much of the dispute as to whether the normal law is realized in fact, depends on just how much departure of fact from theory we allow without giving up the theory. Von Kries could call normal in 1886 what Urban would see as asymmetrical in 1910: standards have gotten more rigorous.

[47] *Op. cit.* 10-83.

brings together the experiments on the drawing of balls and the like, and shows that with increasing number of cases the relative frequencies lie within a decreasing *Spielraum* and thus in a sense approach the expected frequencies. He then goes on to show that the frequencies of social statistics behave in a manner similar to the frequencies of the experiments on "chance." He is not so much interested in the applicability of the normal law as he is in the facts. The appeal to facts is, however, always a protest against theory which is given *a priori.*

After all the facts are the scientific business. We have traced what seemed to be a growing realization of the logical inadequacies of a theory, but have we perhaps not been witnessing a deepening appreciation of the facts? When anthropometric measurements were first being made, there stood out the fundamental fact of the massing of cases about the average, the rapid falling off of more extreme frequencies, and the extreme rarity of widely divergent cases. To this extent the normal law was the fact. But as interest centered upon the details, the inadequacies of so simple a generalization became apparent. There was effort enough at reconciliation, but, in general, science kept to the facts, and a more flexible system of representations came into use.

There is a pretty instance of the inadequacy of the normal law to scientific description in a research of Pearl's.[48] Pearl studied statistically the number of leaves in a whorl in Ceratophyllum. He found that the distributions for number of leaves were skewed one way at the proximal end of the main stem or of a branch, the other way at the distal end, and that the form of the distribution passed through symmetry somewhere between the two ends. Thus he writes:

> The phenomenon of skew variation stands forth in this case, free of doubtful interpretation through selection or any similar factor, clearly and definitely as a phenomenon of growth. In the face of facts of this kind it is difficult to understand how anyone can be so firmly convinced of the *Allgemeingültigkeit* of the normal or Gaussian law, as some biologists still are . . . Skewness in variation is a very real biological phenomenon, which may be changed and modified, not only in degree, but in direction, by various biological factors like growth, as, for example, in the present case.

In other words, if nature is aiming to make something follow the normal law, then, as the stem of Ceratophyllum grows, she changes continuously that something which is to fit the law. The real problem of scientific description is an account

[48] R. Pearl, *Variation and differentiation in Ceratophyllum,* 1907; especially 90f.

of nature's changing. To assume the normal law in such a case would be to shut one's eyes to the fact.

Here we may leave the question of the *a priori* nature of the normal law. There is, after all, no magic in it. It gives us back always what we put into it. If we know from experience what nature is up to, as we do with the coin, then we can proceed upon cogent reasons to apply the law and we get results. If we do not know, we must appeal to nature and see. We have then no reason to expect that we are going to find the law before the appeal is made, whereas we have considerable reason to expect not to find it, since the form of distribution depends on the unit of measurement and we have arbitrarily chosen one of a possible infinite number of units. When we do go to nature we find all degrees of resemblance to the law and divergence from it, and we may even find that the degree of divergence from the normal becomes the significant fact of our observation.[49]

3. The Relation Between the Unit of Measurement and the Form of Distribution

We have seen that if we alter the unit of measurement we alter the form of the frequency-distribution, and that if the change is in accordance with any function other than direct proportionality we get alterations of the skewness of the curve. Bertrand made the point with especial clearness, and it is strange that a relation so obvious should have received so little attention.

Williams recently ran upon the difficulty when experimentally he obtained psychometric functions for memory.[50] He

[49] We have noted that the universality of the normal law was contested by the following persons: Bravais, 1845; Quetelet (admitted Bravais' argument), 1846; Lexis, 1877; Galton, 1879; McAlister, 1879; Bertrand, 1889; Pearson, 1895 and emphatically in 1900; Pearl, 1904; Urban, 1910; Edgeworth, limitedly in the *Britannica*. The list is by no means complete. We may add: R. L. Ellis, *Philos Mag.*, 3 ser., 1850, 321-328; Jevons, *op. cit.*, 1883, 374ff.; Venn, *Nature*, 36, 1887, 411ff. and *Logic of chance*, 1888, 23-52, 501ff.; Fisher, *op. cit.*, 1917, 149. Urban, *op. cit.*, 241, mentions Helmert, Laurent, Bessel, Guarducci, and Edgeworth as demonstrating asymmetry of distribution. Fisher, *loc. cit.*, refers to Oppermann, Cournot, and Dormay (especially Dormay) as showing the inadequacy of the Gaussian dogma. Urban's serious consideration of Pierce's data as showing symmetry is hard to understand for Pierce's symmetry was presumably assumed in the first place. His beautiful curves must have been fitted, for all he says of them, by eye, and when one attempts to treat his data statistically they do not check. *E. g.*, the numbers of cases for days 20 to 24 are respectively 495, 494, 502, 499, 498, and 497. Each should be 500. C. S. Pierce, *Report of U. S. Coast Survey*, 1870, 212ff.

[50] H. D. Williams, *Amer. J. Psychol.* 29, 1918, 219-226. See also Boring, *Psychol. Bull.* 15, 1918, 32f.

expressed the frequencies for recall of a material as a function of the number of repetitions of the material, but he felt, for certain cogent reasons that the student of memory will appreciate, that the repetition was not a satisfactory unit in terms of which to work. The psychophysical effect of an early repetition is presumably greater than that of a late one and Williams would have liked a more truly mnemonic unit. If he could have taken the normal curve *a priori* as the necessary function, he could have adjusted his results to it and have discovered a mnemonic unit that was some particular function of the number of repetitions. Such a relationship would have provided a knowledge of the effectiveness of successive repetitions and would have produced a method for use with other mnemonic measures. All measures eventually could have been interrelated, and a great advance made in the psychophysics of memory. Unfortunately, however, there was no justification for assuming the normal law, and, with both law and psychophysical unit unknown, Williams, as he realized, was placed in the dilemma of being able to reason from neither to the other. The way out, of course, was to give up the impossible, and to stick to the observed facts, leaving the frequencies to stand as a function of the number of repetitions.

Trabue, in attempting to establish a language-scale "with equal distances between steps," ran into the same difficulty.[51] His material,— sentences to be completed by the Ebbinghaus method,—had no quantitative aspect except the number of arbitrarily chosen completion-tasks that could be successfully accomplished. He chose, therefore, to assume the normal law and to calibrate his material by reference to it, taking the probable error as the unit.[52] His hint of a cogent reason back of the normal law stands quite unsupported.[53] Now the language-scales presumably do "work;" but if they do, it must be for the reason that this form of measurement, pretending to nothing more accurate than Quetelet achieved, counts his degree of rigor successful.

[51] See foot-note above, p. 12.

[52] Pp. 30ff. There is a further, somewhat obscured assumption, *viz.*, that the P. E. for one age equals that for another: see pp. 54ff. That the dispersion would not alter with age would hardly be expected; *cf.* J. B. Miner, *Deficiency and delinquency*, 1918, 252ff. At any rate the assumption is gratuitous, unless, as is always true, we argue that the unit is the premise in the case, and that premises are indisputable. The law of insufficient reason is usually applied to personal predilection.

[53] A foot-note on p. 31 says of the normal law: "If this assumption is made the results that follow are in . . . close accord with known facts;" but there is nothing more.

These two papers are but examples of how the problem of the unit arises in very different fields of mental measurement and how their solution is balked by an inability to assume in advance some distributive function like the normal law.[54] In such cases the limitation is pretty clear, for we have to make an assumption and it takes but little argument to show that an assumption in itself can not be fact.

There is, however, a more subtle way in which the will to believe in the normal law gets in its work. Suppose we give up assumption and go to experience, in order to study a special group of related phenomena; and suppose we find that very many of these phenomena, as we measure them, are distributed every one in accordance with the normal law? May we not conclude by analogy that the same law applies to other phenomena? If a penny comes heads and tails equally often, will not a silver dollar? If stature follows the normal law, what about length of forearm?

The answer is that it all depends on how much one knows of the nature of the case. One may reason from the penny to the dollar, if one knows — inductively, from experience — that homogeneity and shape in the penny are the attributes that condition the law, whereas size, chemical constitution, color, *etc.*, are irrelevant. (Even so one may make a mistake; a magnetized steel disk in a magnetic field might give unexpected frequencies.) To reason from stature to length of forearm, however, requires that we know that the size of the forearm is conditioned in the same way as is the total length of the body. It is not enough to know that both fore-

[54] There is no need to assemble a list of studies where the error in question is committed. Often it is present only by implication. Pearson, for example, in his study of "The relationship of intelligence to size and shape of head, and to other physical and mental characters," *Biometrika* 5, 1906, 105-146, assumes the normal curve in order to provide a scale on which his results may be plotted. He is careful to point out that the assumption is merely for convenience and that it does not affect the chief business of the paper, *viz.*, the computation of correlation ratios (pp. 106f., 118). Very well. But then Pearson proceeds to set up a scale of units called "mentaces," with every unit 1/100 parts of a particular group that about equals the standard deviation. A complete scale of mentaces is suggested, ranging from —300 to +300. Offhand it looks like measurement, although the careful reader discovers that it is merely an illusion resulting from overindulgence in the normal law.

Miner's discussion, *op. cit.*, 254-276, is here to the point. Miner sees the problem of the unit (255f.) and combats the Gaussian superstition (267ff., 272-276). His strictures on Pearson (272ff.) are tolerant. He does not seem, however, fully to appreciate (in spite of pp. 255 and 273f.) that the problem of the unit and of the form of distribution are one and the same.

arm and stature depend on the same factors; we must know that they depend upon them in the same way, that is to say, we must know that we are dealing with the same scale of growth. If the forearm were to vary as the square of x and stature as the cube of x, then prediction that the distribution of the one would be like the distribution of the other would be doomed to disappointment. With the coins, we know that the scales are comparable; each is expressed in percentage of heads. But in biological and psychological phenomena we can scarcely hope for any such certain analysis of the essential conditions.

Apparently then sometimes we may generalize and sometimes not. Can we make out a list of the classes of phenomena to which it is safe to say that the law applies? At least we may try.[55] And we shall have to consider five classes.

1. In the first place there are the *Glückspiele*,— coin tossing, card drawing, dice throwing, roulette, and the persistent urn or bag with the red and white balls inside, that introduces all students to the theory of probabilities. In such cases the presumption for the validity of the law is stronger than anywhere else. It seems that the red and white balls ordinarily do come out of the bag about equally often if they exist in equal quantities inside. Westergaard in 10,000 drawings got 5,011 white balls. Yet Pearson found that Weldon's 26,306 throws of dice exhibited "a bias toward the higher points." He calculated that the chances of 5 or 6 coming up on these dice exactly one-third of the time were less than two in 100,000, but that the chances of 5 or 6 coming up .3377 of the time

[55] What is needed is a comprehensive statistical study. If some one would get together only a hundred distributions based on large numbers (physical, biological, and mental distributions as found in nature, and distributions from such things as games of chance, tide tables, incommensurable numbers, *etc.*, as found in human civilization), and if he would treat them all accurately *by the same method*, applying some criterion of goodness of fit to the normal law (*cf.* Pearson, *Phil. Mag.*, *op. cit.*), we should begin to have some notion of the way in which all these things do behave. But the inexperienced statistician should not begin lightly on such a task. He must be prepared to use much judgment before results taken under very different conditions are comparable. The very decision as to the size of the class-interval in every case is tremendously baffling; and there are questions of weighting that are equally bothersome. When this task is done he ought to fractionate and do it all over, in order to see how the principle of the *Spielraum* as dependent on the law of large numbers works out. And after that, he has left the study of the effect of changes in the scale of units. How does the goodness of fit alter when the logarithm or the square root of the measure is substituted for the measure, and how much does it alter and under what conditions does it alter most? It is a large order and one that is well worth filling.

were as great as two in 16. "Oh well," we say, "you could not expect any real dice to be exactly homogeneous. There is not much difference between .3377 and ⅓." No, and in any real experiment ideal conditions are never exactly realized. All experimental observation is only as accurate as the control of the conditions of observation.[56] The tendency with Pearson's method is to conclude from particular cases against a generality by a method more precise than the precision of the observed cases allows.[57] In general the *Glückspiele* seem to follow the law as well as the faulty conditions of their construction and operation permit.[58]

2. It is generally maintained that the occurrence of digits in the expansion of an incommensurable number like π. ϵ. $\sqrt{2}$, log 7, *etc.* follows the normal law, and that all digits in the long run occur equally often. Here the law of insufficient reason is peculiarly tempting. If there is no relation at all between the intrinsic properties of a circle and the number of fingers on the two human hands which determined the decimal system, then why should not all digits turn up equally often in the expansion of π? There is no reason why they *should* not. The *fact* is that in the first 707 digits of the decimal of π the sevens occur only three-fourths as often as they should, although the other digits do better by the law.[59] This case, however, is but a single one,[60] and, as Venn points out, the chances of one digit's deviating from equality as much as do

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[56] Of course we *want* the die to come up equally often on every face. When it does not, we would rather blame the die than the law. But, though such a gaming 'instinct' may be built into human nature, it is nevertheless an insufficient reason for science. To blame the die because it does not fit the law is unfair; but to say that the die because of practical conditions does not disprove the law or even that it does approximately prove it, is allowable.

[57] But this is a paper in itself.

[58] For summaries of results, see Westergaard, *op. cit.*, 21-38; Pearson, *Chances of Death,* 1897, i, 11-25, 44-62. The failure of Monte Carlo to obey the law of chance seems not to apply to all roulette; *v.* pp. 57f. Westergaard's experiment is in *op. cit.*, 36f.; Pearson's computations are in *Phil. Mag.*, 5 ser., 50, 1900, 167ff. See also Fisher, *op. cit.*, 127-145.

[59] The actual frequencies, which Venn who discusses them (see note below) does not give, are: 0, 73; 1, 78; 2, 72; 3, 74; 4, 74; 5, 63; 6, 68; 7, 53; 8, 73; and 9, 79. The expansion of is by W. Shanks, *Proc. Roy. Soc.* 21, 1873, 319.

[60] There is no reason why a division of a decade by a trinity should be prepotent of other trinities, that 10 divided by 3 should be 3.33333 . . . (in the decimal system; the duodecimal system gives a completed two-digit quotient); nor is there any reason why the product of 12345679 by 27 should be 333333333, *except* the absolutely adequate reason that such is the inherent nature of the case. But in the realm of probabilities these exceptions help to prove the rule.

the sevens is one in four.[61] Edgeworth found that there was one chance in ten that the first 1,800 digits of the expansion of the fraction 1/1861 were following the law of chance.[62] Both he and Venn are convinced, however, that the rule of the coin is applicable to such series of digits, and indeed such is the presumption though the generality is here less well established than for games of chance.[63]

3. In 1904, long after the publication of his first papers on skew variation in homogeneous material, Pearson made the following statement:

> [Suppose] we assume a certain distribution of frequency for the character in human populations. This distribution of frequency is given by the Gauss-Laplacian normal curve of deviations from the mean. . . . Now the problem before us is the following one:—Is this assumption legitimate? It certainly is not true for organs and characters in *all* types of life. But it really does describe in a most remarkable manner the distribution of most characters in mankind . . . I should be the last to assert that no human characters can be found that do not diverge sensibly from this Gaussian distribution. But I believe they are few, and that for practical purposes we may with nearly absolute safety assume it as a first approximation to the actual state of affairs.[64]

Everything hinges here on the word "approximation." The case is very different from that of the coin or of the incommensurable number. We do not know anything about the units with which we are working except that they are the units with which we are working. When we change from the penny to the dollar we keep the same scale, one of proportions of heads in a two-fold universe; when we change from π to $\sqrt{2}$ we also keep the same scale, one of proportions of digits in a ten-fold universe, but when we change from stature to the forearm we have no guarantee that the inch means the same thing biologically. And we must use a biological unit, if we are to bind our class together by a biological concept, if we are to predicate the normal law of all *human* characters. That the form of distribution is a function of the unit is unescapable. What we need to know, and what we do not know, is whether the differences that would occur with biologically reasonable

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61 Venn, *op. cit.*, 111-118, 247f.

62 Edgeworth, *Cam. Philos. Trans.* 20, 1905, 128-130.

63 See also S. Newcomb, "Note on the frequency of use of the different digits in natural numbers," *Amer. J. Math.* 4, 1881, 39f.

64 *Biometrika* 3, 1904, 142f. Many curves are given which *look* like good fits. The measures of goodness of fit obtained by Pearson and A. Lee, *ibid.* 2, 1903, 357-462, show great variation. There are equal odds that the statures of "mothers" follow the law, and only a few chances in a thousand that the forearms of "fathers," "sons," and "daughters" do. Pearson seems here, as we should like him to do elsewhere, to take his own criterion with a grain of salt.

variation of the biological unit from the physical unit are small enough to be included within the "approximation." A certain amount of alteration of the scale of measurement still leaves a sensibly normal curve. Have we any cogent grounds for assuming "with nearly absolute safety" that "the actual state of affairs" is not enough differently conditioned from those that we know about as to render it widely divergent from the normal law? This is a reasonable question, and within approximate limits it may be answerable. Certainly we may not generalize with Pearson on any other basis, and certainly Pearson neglects to provide this basis.[65]

4. On such an insecure structure does Pearson attempt to build further. If the physical is normal, why not the mental?

> I put the problem to myself as follows:—Assume the fundamental laws of distribution which we know to hold for the physical characters in man, and see whither they lead us when applied to the psychical characteristics. They must: (a) Give us totally discordant results. If so, we shall conclude that these laws have no application to the mental and moral attributes. Or, (b) Give us accordant results. If so, we may go a stage further and ask how these results compare with those for the inheritance of physical characters.[66]

(The paper is on inheritance.) The assumption of the normal law gives results for the mental characteristics that look like the results for physical characters. Hence we may go ahead with our correlations in each case to indicate the degree of inheritance. The assumption accomplishes little beyond providing the scale for the printed graphs of intelligence; but that was in 1904.

In 1914 we find Pearson still arguing for a normal distribution of intelligence, and the occasions for his argument are certain empirical results which did not show a good agreement with the "Gaussian" law.[67] He is presenting the results of the application of the Jaederholm form of the Binet scale to school children of Stockholm. The samples are very small (261 normal and 301 defective children). There is one chance in 60 that the normal children give but a random deviation

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[65] He is, in fact, not clear on the part played by the unit of measurement. He is concerned much over the normal law and but little over the arbitrary scale of measurement that determines in advance the distribution.

[66] *Biometrika* 3, 1904, 147. We have seen that Pearson also assumes the "Gaussian" curve for mental ability in *ibid.* 5, 1906, 105ff.

[67] Pearson and G. A. Jaederholm, *Mendelism and the problem of mental defect, II: On the continuity of mental defect,* 1914; Pearson, *Mendelism and the problem of mental defect, III: On the graduated character of mental defect, etc.,* 1914. These papers are viii and ix in *Questions of the day and fray.* On adjustment to the normal curve see II, 28-36, 46f.

from a properly "Gaussian" distribution; there is one chance in 20 that the normal children plus an ideal fraction of the defective children are properly distributed in the "Gaussian" manner. Eight-year old children taken alone, however, do show an even chance for the applicability of the "Gaussian" law. This is slim evidence, but it appears that it is not yet time to declare the Gaussian image a false god:

> There is absolutely no reason why the Gaussian curve should be dogmatically asserted to apply to frequency-distributions of intelligence . . . Still there are *a priori* suggestions that it should be tried. In the first place it does describe with a great degree of accuracy most physical measurements in man, and secondly, the Biometric school has found that it gives good results for many measures of intelligence. It is the view of the psychological joint-author of this paper that its comparative failure as applied to the present data lies rather in faults of the tests applied or in the method of applying them than in the non-Gaussian character of intelligence when adequately measured.[68]

This quotation is as near a confession of what the Biometric School is trying to do as we are likely to get. It is frankly seeking always to see Gauss in nature. Its excuse in the case of mental phenomena for a prejudice in favor of this one of an infinity of possible distributions is that the Gaussian curve "does describe most physical measurements in man," and that "it gives good results for many measures of intelligence." The physical agreements, however, are approximate, as we have seen, and the facts with respect to intelligence are questioned.[69] The real difficulty comes, however, in the grounds for analogy. The implication is that we may reason from the physical to the mental because, in this case, both are "human," and that we may reason from one test to another because both are tests "of intelligence." Now we have no distributions of 'humanness,' but only of measurements of human characters; and we have no distributions of intelligence, but only of particular measurements of intelligence.[70] Moreover, in every case the measure is arbitrarily chosen; and it is not even strict-

[68] *Opp. citt., Mendelism, etc., II*, 46.

[69] Miner, *op. cit.*, 267-279, reviews the evidence for a normal distribution of tested intelligence, and concludes: "In spite of these arguments and the evidence of asymmetry of measurements at least of some periods of life it is to be noted that current opinion is probably contrary to this hyposthsis [hypothesis, the normal law], although, as I believe, because it has been mainly concerned with those who are not of extreme ability:" p. 275.

[70] *Cf.* here Miner's distinction between "physical units" and "more equivalent biological units," *op. cit.*, 255. We have noted that Miner does not take advantage of the full force of the distinction; the physical unit may be irrelevant to the problem in hand.

ly a measure, in the sense of being the sum of equal units, unless we make it so on *a priori* grounds. In an intelligence-test, for example, we have no notion whether the difference between 99 and 100 points is the same difference in intelligence as the difference between 9 and 10 points, unless we are ready to define intelligence arbitrarily as the ability to achieve points in that particular test.[71] Now if we define intelligence with respect to one test, we can not assert *a priori* that any other test is a measure of intelligence, and should therefore show the same distribution. *A posteriori* we may show by correlation that a second test measures an ability similar to intelligence as defined by the first test, but in such a process we demonstrate empirically the similarity of distribution. There is no valid reason for expecting normality in one intelligence-test because it has been found in another; and the same reasoning applies to the argument from the physical to the mental.[72]

We are very far, then from a general conclusion that intelligence, a mental capacity comprehensible apart from the particular instrument by which it is tested, is normally distributed. Still farther are we from stating that mental abilities, whatever their nature, follow the normal law.

5. We have to investigate one more class of phenomena, the psychophysical judgments. In this case no claim for generality has been presented, but the trend of the facts bespeaks attention. Urban found that the phi-function of gamma is a good hypothesis for the psychometric function in lifted

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[71] No one who has ever "made up" an intelligence test ought to have any difficulty in appreciating the arbitrariness of the calibration. The tasks of the test are in the first place sheer casual invention. They are put into a scale and tried out. Alterations are made to "improve" the frequencies of success in different component parts, but no one can work accurately from the frequencies to the scale without presupposing the form of distribution. There is never any attention paid to making equal increments of intelligence measured by equal increments of the scale, for the excellent reason that no one has any such definite quantitative conception of intelligence. To call a year in mental age a unit is a fallacy of this sort. See Pearson, *Mendelism, etc.*, II, 36*ff.*; Miner, *op. cit.*, 260ff. There is no evidence (nor are the physical growth curves in any way relevant) that annual increments *of intelligence* are equal in any other sense except in that they are annual. Of course a year is a year, if "annual-ness" is what we are after.

[72] Pearson has appointed himself "watch-dog" of "logic in scientific procedure", *Mendelism, etc.*, III, 3f.; but he seems to need to make friends with one sophistical wolf ("an appearance of knowledge where we are as yet in a state of ignorance") in order to attack another.

weights and in certain acoumetric experiments.[73] Now the phi-function of gamma is simply the integral form of the normal curve. To say that the phi-function of gamma is the psychometric function is equivalent to saying that the dispositional variations of the psychophysical organism, when measured in a scale of units proportional to the scale in which the stimulus is measured, follow the normal law. If the lifted weights are measured in grams and the psychometric function approximates the phi-function of gamma, then the organism varies in the amount of its disposition for judging "heavier" or "lighter" according to normal law, provided always that the amount of this disposition is measured in grams. This relationship is scarcely obvious and the reader may need to read elsewhere,[74] before he can accept it. The essential thing to understand is that the applicability of the phi-function of gamma means that the organism is varying somehow in accordance with the normal law, but that the normality of its variation is as much a function of the unit in which the variation is measured as it is in any of the other cases which we have considered.

Now can we generalize? Can we ever say that all psychophysical judgments tend to obey the normal law? Urban makes no such attempt; rather is he against theorising and for description. "The nature of the dependence," he writes, "is not known and cannot possibly be deduced by any considerations *a priori*;"[75] and he refers us for the function to "the results of observation." This, to the scientist, is a refreshing return to facts from the realm of logic and mathematics; but is not final. Something can be said for generalization.

Unfortunately for ease of exposition, there are two kinds of psychophysics. In the one kind stimuli are presented and the observer reports upon the sense-impressions. In the case of the stimulus-limen, he simply reports sensation or its absence. He is making a brief introspection. In the case of the differential limen he makes a judgment upon the relative degrees of the two sensations, but the judgment is actually little more than a comparative description; the ob-

[73] On lifted weights: Urban, *op. cit.*, 257ff.; *Arch. f. d. ges. Psychol.* 16, 1909, 168-227, espec. 224f. On acoumetry, H. Keller's experiments, *Psychol. Stud.* 3, 1907, 49-89; and Urban's discussion of them, *Arch. f. d. ges. Psychol.* 18, 1910, 400-410. Keller used Wundt's fall phonometer, and the all-important unit of stimulus, which is only just mentioned, is the cm. The psychometric functions of the lifted weights are for grams.

[74] Boring, *Amer. J. Psychol.* 28, 1917, 465ff.

[75] *Psychol. Rev.* 17, 247.

server may be said to be introspecting here too.[76] In the other kind of psychophysics the observer's attention is directed upon the stimulus and not upon his own mental processes. In no sense does he introspect. Always he is making judgments of the stimulus. In introspective psychology this attitude is called the "stimulus error," but in psychophysics it may be in place. Urban's experiments are of this kind,[77] as has been much other psychophysical work. There is reason to believe, however, that psychology will profit more in the long run by work performed under the first attitude described.

In the case of the "introspective" psychophysics we are not to expect a generalization, because everything depends on the unit. Were the psychometric function to approximate the phi-function of gamma ten times, there would be no reason to expect it the eleventh. Suppose the eleventh case were the differential limen for auditory intensity with the Fechner sound pendulum as the instrument. Will the normal law hold? Perhaps when we use as the unit $\sin^2 (\Theta/2)$. If so, it will not hold if we use Θ as the unit. If it holds for neither, it may hold for some other function of Θ. The reports upon sensations depend upon Θ and the momentary disposition of the organism, but in no way upon the scale of units chosen. It is mere luck if the normal law applies. But whatever luck we get is good luck, for the psychometric function gives us the law of the dependence of dispositional variation upon the scale of measurement which we have chosen. A little knowledge of sensory mechanisms is worth the loss of a generality.

When the psychophysical judgments are directed upon the

[76] *Cf.* the nature of the constant psychophysical attitude set up in S. S. George's experiment, *Amer. J. Psychol.* 28, 1917, 1-37.

[77] In the discussion of Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.* 30, 1913, 113-152 (*cf.* also, *e. g.*, *Amer. J. Psychol.* 24, 1913, 274), Urban would appear to maintain that psychophysics deals with describable mental processes; but the implications in the accounts of his experiments, the lack of a reproduction of definite instructions given the observers, and the failure to discuss the introspective competence of the observers, all indicate that the observers were, or at least may have been, making judgments of stimulus rather than of mental process. See *The Application of Statistical Methods to the Problems of Psychophysics*, 1908, 5, 14; *Arch. f. d. ges. Psychol.* 15, 1909, 264f. In *Psychol. Rev.* 17, 229, he speaks of "the judgments of a subject who compares two stimuli." And indeed in the systematic schema of *Arch.* 30 (*op. cit.*) he takes account (1) of mental contents and processes, (2) of physiological conditions and processes, and (3) of physical conditions and processes, but leaves no place for meanings, awarenesses, the stimulus in relation to the subject. It is doubtful if Urban sought at all to distinguish between *Beschreibung* and *Kundgabe* in the psychical aspect of the psychophysical system.

stimulus it may be that we can generalize, for now the scale and the judgment are related. With the sound pendulum we might be asked to judge the height of the fall, which is measured by sin ^{2} $(\Theta/2)$, or we might be asked to judge the angle. In the two cases we would be judging entirely different things, and it is quite possible that the normal law might apply in each. If we found that it applied in a large number of cases, then we might generalize by saying that errors in the judgment of a stimulus, that is to say, errors of observation, are distributed normally. This conclusion, indeed, is Urban's: "We thus obtain the remarkable result that the foundations of the theory of the errors of observation are found in the theory of psychophysical measurement."[78] In other words we would demonstrate psychophysically Gauss' original assumption that positive and negative errors are equally likely.

If there is any general answer to the question: When can we generalize?— it is this. We may expect the normal law to hold within an entire class of phenomena, when it holds for a number of cases within the class, provided always that all variations of members of the class are capable of being expressed as variations of some common denominator. In other words, we must be able to see the same thing varying in different situations. With the penny and the dollar, the law holds for the turning up of heads. The things that make the penny different from the dollar, we know to be irrelevant. In lifted weights and acoumetry we may know, by a control of attitude, that we are studying, not kinaesthesis and auditory sensation, but errors of observation. If Urban did know this, then he was supplied with the only possible ground for expecting the one result on the basis of the other.

4. The Logical Possibilities
(Conclusion)

At last we are ready to take account of stock! There are four logical possibilities, but the circumstances of the case are such that it appears unlikely that the actual mental measurement can make much use of more than one. Let us see.

1. In the first place, there is the possibility of determining *a priori* the form of distribution. This course, applied to the normal law, is popular, but it is usually without sanction. In quantitative psychology we should resist it. We may not assume the normal law in an interpretative study except upon

[78] *Psychol. Rev.* 17, 243; but Urban is being betrayed into generalizing. His statement is based only on the lifted weights and perhaps the acoumetric experiments. The validity of generalization under this attitude is hardly to be demonstrated so easily.

valid grounds. We have seen that the law of insufficient reason furnishes no such grounds. Knowledge simply does not come out of ignorance. The only scientific grounds that are presumptive of the normal law are an intimate knowledge of the constitution of the particular case and of the function that frequencies of occurrence are of that constitution. Such a function comes in science from experience, and, whenever it is thoroughly understood, from experiment. On such cogent grounds we may conclude *a priori* to the normal law or some other form of distribution, and we are following such a logical process in the few cases where we find it legitimate to generalize about the form of distribution for a given class of phenomena.

Two corollaries follow.

(a) If we knew that the normal law had to hold in a particular case, then we could experiment with an arbitrarily chosen unit and determine, by working from the law, the function that the "true" psychological unit, which is following the law, is of the arbitrary unit of the experiment. Any such hope, however, is bound to be illusory, for a knowledge of the constitution of the case involves *a priori* the knowledge of this unit. We may say that, if the dollar is like the penny, we shall get 50% heads; but we can not reverse the argument and say that, because we get 50% heads, the dollar is like the penny.[79]

(b) There is at present very little prospect in the field of mental measurement that our knowledge of the psychological constitution of a mental function or process will be sufficient to enable us to begin work upon a problem with the assurance that the resulting distributions must be normal. There is a bare hint of an exception, that awaits verification, in the psychometric functions of errors of observation. But the evidence indicates that instances, where there are cogent reasons for assuming the normal law in advance of empirical determination, will be extremely rare for a long time to come.

2. We may begin in mental measurement with the psychological unit, for if we can not determine the unit from the distribution, we may, nevertheless, determine the distribution from the unit. Such, in fact, is the necessary scientific order.

[79] *I. e.*, in physical constitution. We are not denying inverse probability, but insisting merely that a statement of physical constitution is more complex than a statement of frequency and that the same frequency may be variously conditioned. If we know only that we get 50% heads, we can not reason that the underlying physical constitution is that of the dollar or the penny. For all we can tell we may be dealing with an asymmetrical loaded die, with one head and five tails, that belongs with the penny only in the matter of frequency of heads and not in general physical constitution.

The great difficulty is, as we have just pointed out, to find anything that we may properly call a psychological unit. The sense-distance is such a unit. It is a unit of measurement that is mental *per se*.[80] Most of the sensory work in psychology, however, has had to do with the determination of limens and has not made use of the mental unit at hand. The extensive psychophysics of memory has not achieved a psychological unit, nor have the other departments of the psychology of process. In the psychology of capacity or function the case is worse. For example, with intelligence, which is the mental capacity most often measured, we have seen that, not only is there no attempt to make equal increments on an intelligence scale correspond to equal increments of intelligence, but that the concept of intelligence is so vague that any such accurate quantitative relationship is in practice almost meaningless. We are not, however, at our rope's end.

3. If a psychological unit is not to be achieved we may use a "physical" unit, that is to say, some arbitrarily chosen aspect of an arbitrary scale of measurement. Such units are the year (mental age), the second (mental tests where time is the measure), the item or task (where the number of points is the number of unit tasks completed, *e. g.*, the U. S. Army intelligence tests), the gram (lifted weights), the syllable (memory experiments), and so on. Every such unit is arbitrary. There is no evidence that equal increments of its scale correspond to equal increments of the psychological entity measured. When we define intelligence, in order to gain definiteness of conception, as ability in some particular test, we are simply substituting a "physical" unit for the ideal, but unachievable, psychological unit. Now the application of a unit that is not psychological to a quantity that is psychological does not yield a measure of the quantity. It will place the quantity in a given position upon the arbitrary scale, and will determine the rank-orders of a number of quantities so placed, but the assumption that rank-orders tell the amount of a given quantity, or, if the zero be unknown, of a given increment, is, of course, unwarranted.

There is one case, of frequent occurrence in the behaviorism

[80] *Cf.* E. B. Titchener, *Experimental psychology, Quantitative student's manual,* 1905, xix-xxxvii. The thing is so simple when it is seen! In physical science the nature of the unit is given in the presuppositions of the science, and in psychology it is given mentally in the sense-distance. It is the indefiniteness of the mental function that makes trouble and gives rise to the attempt to create a unit in retrospect by an appeal to the hypothesis of the normal law. Since this paper deals with the appeal to the normal law, we may, however, pay homage to the sense-distance and proceed.

of mental tests, where the "physical" and psychological units seem to become identical. Suppose we are studying the learning of typewriting and express our results in the number of words written per minute. Do not words-per-minute truly measure ability in typewriting? Here we are involved in a question of point of view. Words-per-minute measure the product of typewriting. They may perhaps be said to measure ability in typewriting from the point of view of the employer of a stenographic force; they are the ability of his office force over against a given job. A behavioristic psychology that identified behavior with mere product, without any reference at all to the conditions of the product, might take the point of view of the employer; but such a psychology would achieve only a physics, or more likely a common sense, of typewriting. Any definition of behavior as *response to a situation* [81] brings in the behaving organism and changes the ground. We can not say that words-per-minute truly measure the behavior of the psychophysical organism or its response to the situation. Does an increment of ten words per minute mean the same change in ability or in behavior or in response when added to an ability of 50 words per minute as when added to an ability of 100 words per minute? To the employer, yes; but not to the psychologist, or, presumably, to the organism. It is indeed an heroic measure, when we can not make the unit psychological, to attempt to a-psychologize psychology.

4. We are left then with the rank-orders of our psychological quantities, given by reference to a fixed but arbitrary extra-psychological scale; and it is with these rank-orders that we must deal. We are not yet ready for much psychological measurement in the strict sense. It is true that psychophysics may yield a unit and also true that ordinarily it does not. The sense-distance has had only a limited use, and we do not yet know that the stimulus-distance (for it is upon stimulus-distances that the psychometric functions of errors of observation are founded) constitutes a true unit in a behavioristic psychophysics. The case, however, is not so bad as it seems at first. There is nothing new in the contention that mental measurement is impossible,[82] whereas now we do gain the assurance that rank-orders at least are validly demonstrable. And there

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[81] *Cf.* the point of view of behavior presented by E. B. Holt, *The Freudian wish,* 1916, espec. 153ff. Behavior is *what* the individual is *doing,* not that he is moving. We can perhaps measure movement, but we have yet to conceive the unit for "doings."

[82] Indeed, in confining ourselves to a consideration of the part played by the normal law, we are leaving a famous controversy untouched. *Cf.* O. Klemm, *A history of psychology,* trans. 1914, 150-155, 232-267.

is a great deal that can be done with rank-orders. We can deal with frequencies, medians, and quartiles. For example, it is considerable to know of two groups that the lower quartile of the ranks in one overlaps and is practically coincident with the upper quartile of the other. What we must remember, however, is that we are dealing with the statistics of medians, quartiles, contingencies, and correlation ratios; not with the statistics of averages, standard deviations, coefficients of correlation, and linear regressions. All those statistical constants, that imply a scale of equivalent units, violate in use the conditions of the case and lead to a precision of result that is an artifact. The serial constants, that do not presuppose a unit, yield less intricate resultants, but they present a rougher picture that represents truly the rough material which they describe.[83]

The initial error in the application of the theory of probabilities was the assumption of the law of insufficient reason. It was wrongly supposed that knowledge could somehow be wrought out of ignorance. This very error, however, has never been routed. It has gone on, multiplying mischief. The substitute for insufficient reason is cogent reason. The more we know of the intimate nature of the entity with which we are dealing the more accurate and complete can our descriptions become. But, if in psychology we must deal—and it seems we must—with abilities, capacities, dispositions and tendencies, the nature of which we can not accurately define, then it is senseless to seek in the logical process of mathematical elaboration a psychologically significant precision that was not present in the psychological setting of the problem. Just as ignorance will not breed knowledge, so inaccuracy of definition will never yield precision of result.

[83] *Cf.* C. J. West, *Introduction to mathematical statistics,* 1918, 94.

FOVEAL ADAPTATION TO COLOR[1]

By Hubert Sheppard

Contents

Introduction

The primary aim of this study is to describe the course of foveal adaptation to color. We have also obtained records of the times of adaptation, and have attempted to show their variation with variations of intensity and chroma; but our main purpose throughout the experiment has been to secure introspective reports of the attributive changes which occur while the fovea or macula lutea is under stimulation by a color, from the moment of exhibition until the color has disappeared or until there is no further alteration of hue or tint or chroma. The peripheral retina is discussed only in so far as its discussion bears upon adaptation at the fovea.

Wherever possible, all earlier experiments which seemed to relate, directly or indirectly, to our problem were repeated, and their results tested, before our own technique was finally decided upon. Very little work has been done, however, that bears directly upon foveal adaptation considered as a mode of experience. Adaptation in general depends (1) upon wave-length, wave-form, and wave-amplitude of light, and (2) upon the time during which the light-waves affect the eye. Hence the problem may be approached from the side of light-dark adaptation, of color adaptation, or of negative after-image. Previous investigators have studied the first and third

[1] From the Psychological Laboratory of Cornell University.

of these topics rather than the experienced course of color adaptation.

The term *adaptation*, as applied to visual experiences, was first used by Aubert, to denote "the accommodation of the eye to light-intensities."[2] Nowhere in his writings does Aubert connect this term with color-changes, for which he uses the term 'fatigue.' In his paper on fatigue and retinal after-image,[3] as elsewhere, he argues that there is a close analogy of function between the fovea and the peripheral retina. Colored objects, he says, appear colorless in both direct and indirect vision, if the stimulus is sufficiently long continued. He is, therefore, familiar with complete adaptation, but includes it under the term 'fatigue.'

Exner, working under the direction of Helmholtz, made the first systematic study of adaptation (fatigue) to color-stimulation.[4] In an experiment in which he describes the course of the after-image, he finds that three parts of the spectrum are much less altered in appearance by 'fatigue' than other parts, namely, *R* from the end of the spectrum to a point between the lines C and D, *G* between E and b, and *B* near G. He notes also that, if the after-image of a color is projected upon another color of the same hue, this second color appears much less saturated.[5]

Von Kries investigated the problem of adaptation from both the physical and the psychological points of view. He found that "with long fixation, a color loses in brightness as well as in saturation."[6] suffer no change in hue, while the other colors change in the direction of *Y* or *B* from *G*.

Hess made a study of the tone-changes of spectral colors when the retina was fatigued with homogeneous light.[7] He observed that all colors possess either a *Y* or a *B* valence. That is, all colors, from near a pure *R* to a pure *Y*, or near a pure *G* to a pure *Y*, will change under adaptation towards *Y*; and all colors from near a pure *R* to a pure *B*, or near a pure *G* to a pure *B*, will change towards *B*. Further, during the process of adaptation the stimulus-color always becomes less and less saturated, until it disappears; the complement of the stimulus-color always remains unchanged in hue; and the appearance of a spectral color after adaptation corresponds in general to a mixture of that color with its complement.

Burch was able to produce temporary color-blindness by means of general adaptation at any intensity.[8] In an article entitled "On the Production of Artificial Colour-blindness to Successive Contrast" he describes the process of adaptation as complete for *R*, *G*, *B* and *V*. In another experiment he was able to get complete adaptation in 3 min. when the intensity of the stimulating light was bright moonlight. His method for very high intensities was as follows: The eye of an

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[2] H. Aubert, *Physiol. d. Netzhaut*, 1865, 39 ff.

[3] Moleschott's *Untersuch.*, IV, 1858, 220 ff.

[4] J. Aitken, *Roy. Scot. Soc. of Arts, Proc.* 1871-2, and E. Hunt, *Colour Vision*, Glasgow, 1892, made similar studies of adaptation. A detailed account of their method and procedure is given later by Burch.

[5] S. Exner, 'Ueber einige neue subjective Gesichtserscheinungen,' *Arch. f. Physiol.*, I, 1868, 375-394.

[6] Nagel's *Handb. d. Physiol. d. Mensch.*, III, 1905, 213-220.
Further, *Y* of a wave-length of 560 $\mu\mu$, *G* of 500 $\mu\mu$, and *B* of 460 $\mu\mu$

[7] C. Hess, *Arch. f. Ophth.*, 1890, XXXVI, Abtl. 1, 1 ff.

[8] G. J. Burch, *Phil. Trans.*, 191 B, 1899, 1-135. *Roy. Soc. Proc.*, 66, 208 ff, 216-219.

observer was exposed to bright sunlight, which had been focussed by a burning-glass through a colored screen. After the eye had been subjected to this intense light until all sensation of the particular color was lost, the observer proved to be temporarily color-blind to the color used in stimulation. Further, four parts of the spectrum, *R* from A to B, *G* in the neighborhood of E, *B* about half-way between F and G, and *V* at and beyond H, showed marked fatigue-effects, differing in degree but not in kind. All direct sensation of the stimulus-color was temporarily lost: *R* for 10 min., *G* for a still longer period, while the recovery from *V*-blindness might require a couple of hours. Whenever the eye is completely fatigued to any color, that color is absent from the spectrum, whatever the fatiguing intensity may have been. The observer is, in fact, color-blind for the particular color. After fatiguing the eye with three of the four colors, Burch found that only one color remained.[9]

Troland has unpublished data from which he is able to determine a theoretical course for the process of adaptation in all the adapting sense departments. Retinal adaptation follows the same general course as that of the other adapting organs.[10] The rate of the process is greater in the beginning, with a decrease rapid at first, then more gradual, until it finally reaches a level at which it is constant.[11] Troland finds also "that, at least for stimuli of fairly high intensity, adaptation does not proceed to a point at which the resultant sensation reduces to a neutral gray."[12]

In sum, then, Exner, von Kries, and Hess come to almost exact agreement with regard to the gross results of adaptation to color. (1) The 'primary' colors, *Y* 550-574 $\mu\mu$, *G* 495-520 $\mu\mu$, *B* 435-471 $\mu\mu$, do not change in hue under the process of adaptation. Hess finds a psychological *R* outside of the solar spectrum which also does not change. All other colors change either towards *Y* or towards *B*. (2) A color, after adaptation, corresponds in general to a mixture of this color with its complement.

As regards adaptation to color at high intensity, there is difference of opinion. Burch gets complete general adaptation with any intensity which produces temporary color-blindness to the stimulus-color. Troland, on the other hand, finds that color-adaptation at high intensity does not reduce the color to a neutral gray.

Experimental: Plan of the Experiment

The present experimental work upon foveal adaptation to color was begun in October, 1918, and ended in September, 1919. The observers were Dr. C. W. Bock (B), Mr. H. Sheppard (Sh) and Mr. M. J. Zigler (Z), instructors in psychology, and Mr. H. Liddell (L), Miss A. K Sullivan (S), and Miss M. Finck (F), graduate students in the department.

L, S and Z were given several weeks' training before any data were taken for our records; F was relatively unpractised.

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[9] A. W. Porter and F W. Edridge-Green, *Proc. Roy. Soc.*, 85 B, 1912, 435 ff, find somewhat different results for *B* and *G;* but Burch, *Proc. Roy. Soc.*, 86 B, 1912-13, 117-18, argues that their work was not carefully done, and that their results are not strictly reliable.

[10] L. T. Troland, *Am. J. Psych.*, 25, 521 ff.

[11] *Ibid.*, 508.

[12] *Ibid.*, 520.

Our object, as has been stated above, was to describe the course of foveal adaptation to color under as many different conditions as possible. We do not attempt a theory, nor do we draw any theoretical conclusion whatever.[13] We aim only at a qualitative study of the experienced course of adaptation, with secondary reference to the times of adaptation for the different colors used.

The experiment as a whole comprises three parts. In Part I. we examine general adaptation to a surface color in direct sunlight. Here the experienced course of adaptation is described in its various stages. In this preliminary experiment we also take account of foveal adaptation in a general and tentative way. In Part II. we work with surface-colors, and the process of adaptation takes place under certain controlled conditions. These conditions greatly reduced eye movement, and kept the illumination constant. Here we describe the experienced course of foveal adaptation to 9 different colors, *R, O, Y, YG, G, BG, B, V,* and *C.* In addition we consider the effect of chroma upon adaptation. In Part III. pure spectral colors are used with various intensities of illumination. Here we describe the experienced course of foveal adaptation, and note in what respect this process differs from the course of adaptation when a surface-color is used. Difference of chroma is also considered. Two colors, differing in hue, are matched for chroma, and the process and time of adaptation are recorded in each case. We also give the results of some experiments made with high intensities of stimulus.

Part I.: Preliminary Experiment

General Adaptation in Direct Sunlight

We are here concerned to determine the course of general adaptation when a surface-color, illuminated by direct sunlight, stimulates the entire retina. In former experiments peripheral adaptation to color has been found to be more rapid than foveal.[14] Our main problem throughout the preliminary expériment was the study of the course of adaptation, by introspective observation, as the process travelled over the retina from periphery to fovea. If there is a rapid fading-out of a color under peripheral stimulation, as is indicated in the literature, it ought to be possible to observe this change in its progress towards the central retina.

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[13] Papers on 'An Experimental Treatment and Discussion of the Theory of Adaptation and Color Vision' as well as 'The Early Development of Color Vision' are now under consideration and will follow shortly.

[14] J. W. Baird, *Color Sensitivity of the Peripheral Retina,* 1905, 53-74.

Procedure.—A drawing-board of 28 x 36 in. was fastened in a window on the west side of the optics room. The board served as a table upon which to place the stimulus-color. The following papers were selected for stimuli in the preliminary tests:

Color	Brightness Value		Match to a Gray	Brightness Value of Match	
	Black	White		Black	White
R	288°	72°	211°	88°	272°
BG	206	154	149		
Y	144	216	170	154	106
B	263	97	190		
GY	162	198	125	112	248
V	285	75	235		
C	259	101	208		
G	250	110	132	97	263
B					
O	216	144	92		
G			120	112	248
B			148		

A sheet of colored paper, 19 x 24 in., was spread upon the table. In the center of the sheet was a very small light dot for a fixation-point. *O* sat facing the stimulus with one elbow resting upon the table and the chin in the palm of the hand. This position brought the eye within about 25 cm. of the fixation point, and was found to be convenient for fixation in monocular vision. All the experimental work for this part of the experiment was done during the months of July and August, from 2 to 4:30 o'clock in the afternoon. We were careful to select days when the sky was free from clouds or haze.[15]

The instruction to *O* was to report all change in color sensitivity, from the time the eye first fixated the stimulus until the color either faded out to a gray, showing no trace of color, or reached a stage where no further change in hue, tint,

[15] It is very difficult to get complete adaptation to *R*, *BG*, *B*, *C* and *V* in a reasonable length of time unless the sun is shining brightly. The sky must be free from clouds or haze, or the times of adaptation will vary considerably.

or chroma was perceptible. *O* was asked to give a running report of all sensory change, and in as much detail as possible.

Results.—The results are given in summary for the different observers and stimuli.

Observer B. Stimulus *Y*.—A gray film soon appeared in front of the stimulus-color. This film came in from the periphery. There was a slow change in chroma until the color faded out to a white or very light gray. A small area surrounding the fixation-point persisted longer than the other parts of the field. The change was slow at first, then more rapid towards the last.

Stimulus *BG*.—The outer edge began to pale rapidly at first. This paling extended inward towards the center. The center remained richer in chroma than the outer edge for some time. Finally the surface evened up into a gray field.

Stimulus *B*.—There was no change in hue. The haze appeared over the surface from the outside, although very slowly as compared with some of the other colors. The color persisted for a considerable time around the fixation point. In the process of fading out there were many bluish spots scattered about among white filmy spots. These spots would even up into a white or light gray surface.

Stimulus *R*.—While the color was fading out from the periphery, certain portions did not appear uniformly colored; little filmy red dots were scattered about over the surface. Finally the red spots paled out to gray, and then evened up over the whole surface. The color around the fixation-point remained for some time as a dirty brown color. The final gray surface was very dark.

Stimulus *V*.—There was a rough cloudy surface over the color for a considerable time. The color began to pale out from the outer edge, and the change traveled towards the center. The roughness always smoothed out into a gray film of a thick texture. It is very difficult to tell just when adaptation is complete.

Stimulus *O*.—It faded into a beautiful light yellow, beginning on the outer edge and gradually decreasing towards the fixation-point. From the *Y* it went into a gray which was somewhat darker than the *O*. The steps were perceptible though gradual. All changes were from the periphery towards the center.

Stimulus *C*.—Almost immediately the surface began to grow white in the outer portion of the field. This white gradually approached the center. The final stage was a lustrous silvery gray.

Stimulus *G*.—There was a slow change in hue from *G* through *Y*. The *Y* finally changed into a very light gray, very much lighter than the stimulus-color.

Stimulus *GY*.—The green faded out of the periphery very quickly. This process always took place before the *Y* began to go. The *Y* then changed into a very light gray. The surface of the stimulus-field at times had a rough, uneven appearance. This gradually changed into an even surface without any color.

Observer Sh. Stimulus *Y*.—Almost immediately the color began to grow lighter and the chroma poorer. There was no change in hue. As soon as the color began to fade a filmy surface appeared over the surface of the color. The fading out process always came from without towards the fixation-point. The periphery became white some time before the fixation-point. The final stage was an almost white surface.

Stimulus *BG*.—There was a gradual, though rapid decrease in saturation from the beginning. Also there appeared a very rough, uneven surface about the fixation-point, which was much more satu-

rated than the rest of the field. This area gradually evened up into a gray. The hue changed slightly to *B*.

Stimulus *B*.— The blue faded out very slowly from the periphery to the center. At all times the hue appeared to shine through a white filmy surface which formed over the color. This effect persisted to the end.[16] The complete stage of adaptation was a very pretty whitish gray, like an after-image of a velvet-black surface. There was no hue-change whatever that could be observed. The entire process was a slow fading away of the color into a filmy surface. Two cases of staring blindness occurred during this series of observations.

Stimulus *R*.— The red is unlike any other color in adapting out to a gray. It passes through different stages of hue-change from the first. Almost immediately it passes over into a red-orange, then to an orange of poor chroma, and finally into a dirty, dingy brown, which is very persistent. This brown finally goes into a poor gray, then to a good gray surface.

Stimulus *V*.— Violet is the most stubborn of all the pigment-colors to a complete adaptation in sunlight. There is a feeling of uncertainty as to the judgment, because the violet is almost of the same brightness as the gray surface which persists when adaptation is complete. (The two are so near alike that it is often difficult for *O* to decide when the final stage is reached.) There is a rough cloudy surface at first, which gradually smooths down with time. Two changes in hue always take place. At first there is a tendency for the red to persist; this presently adapts out, and leaves a bluish-violet surface of very poor chroma.

Stimulus *O*.— Orange adapts out very much like red from the second stage of the red, except that the orange is richer in chroma than the red at this stage. When the color reaches the brown stage, it is almost exactly like the same stage in the red, except that the orange is brighter than the red. One effect of staring-blindness was observed, which lasted for three or four seconds.

Stimulus *C*.— Almost immediately the surface began to take on a bright lustrous appearance. This effect gradually thickened, until the whole surface was of a beautiful silver lustre. The hue changed somewhat to blue, thus causing the surface to become darker.

Stimulus *G*.— For the first few seconds the color did not appear to change. Suddenly there is a change, the yellow appears and the green seems to weaken. From this point there is a rapid fading away. The yellow appears to go out first; the green, now much less saturated, fades out into a beautiful gray surface which is very smooth in texture. Two cases of staring blindness were observed.

Stimulus *GY*.— The color is stubborn at first; the green suddenly goes out; and then there is a gradual, though rapid, fading away of the yellow to a beautiful gray surface. After the green passes away, the process of adaptation is very much like that of the yellow. The final stage, the gray surface, is similar to the same stage in the yellow adaptation.

Observer Z. Stimulus *Y*.— The color began to fade in the periphery almost immediately; this caused the center to appear much lighter, and richer in chroma. The small center persisted for some time, although slowly fading all the time. Finally the whole surface of the stimulus-color became a uniform gray

[16] To get the effect of the soft white surface, when adaptation is complete, a Hering blue must be used. A paper that has a glossy surface is very difficult to observe for this length of time.

Stimulus *BG*.— There was no apparent change in hue. A thin filmy mist came in from the edge of the paper, which caused all color to disappear from the periphery. This colorless effect gradually went towards the center, until the whole color disappeared.

Stimulus *B*.— No hue-change. The light or white foggy mist approached the center from the periphery until the whole surface was a very poor *B*, almost gray. This state persisted for some seconds. Finally all color faded away, leaving only a gray field.

Stimulus *R*.— There was always a decided hue-change. It went first to a very poor yellowish *O*, then to a dirty yellow gray. This persisted for some seconds. The final stage is a peculiar gray. It appears to be a little darker than the stimulus-color.

Stimulus *V*.— There was a slight hue-change to *B*. The decrease in chroma was marked at first. However, the final state is difficult to judge, since the final gray and the *V*, very poor in chroma, are much alike.

Stimulus *O*.— There was a change in hue to *Y*, poor in chroma. This change began in the periphery, and traveled towards the center. Finally the center went out, exactly like the *Y*.

Stimulus *C*.— This is a very stubborn color. It appears to be uneven in surface during the adaptation-process. It changes from periphery to center, but not so markedly as the other colors. The rough uneven surface seems to smooth down into the dark gray film as it thickens over the surface. There is a hue-change, but it is difficult to observe in its course. It would appear to change to *B*, although this can not be observed clearly.

Stimulus *G*.— Very slow change to *Y*. This change only comes about when the color has lost nearly all chroma. The film thickens from the periphery towards the center. This gradually conceals all color. The resulting gray surface appears to be a little lighter than the stimulus-color.

Stimulus *GY*.— The periphery began to fade very quickly. For several seconds this part of the field looked like wool, while in the center of the field there was a *YG* of very good chroma. Finally the center began to fade out, rather suddenly, to a good gray surface. The hue changed to *Y*, although the final stage came when the color was very low in chroma.

Observer S. Stimulus *Y*.— The film came over the surface from the periphery in 20 seconds. There was no apparent change in hue. The final stage was a colorless paper.

Stimulus *BG*.—No change in hue. The surface began to fade out from the periphery, all the time going towards the center.

Stimulus *B*.— Same process as the *BG*. The final gray was somewhat lighter than the stimulus-color.

Stimulus *R*.— There was decided hue-change as the color began to grow less saturated by the gray film which came in from the periphery to the center. The color went through a poor *O* to a very weak muddy *Y*. This last stage persisted for some seconds.

Stimulus *V*.— This is a very stubborn color. It is difficult to decide when all the color has faded out. The film began to appear on the periphery very quickly, moving to the center. The final stage appears to be of about the same brightness as the stimulus-color. There is a hue-change to *B*, although it is very difficult to judge.

Stimulus *O*.—The film began in the periphery. For a little, this seemed to hang on the outer part of the color. Finally it moved over

the center. At this time the hue changed to a very poor *Y*. Then the process was the same as *Y* in the last stage.

Stimulus *C*.— The color made some change in hue, but it was so slow and gradual that it was difficult to judge. There was a peculiar uneven surface during the course of adaptation. The cloud appeared from the periphery, moving to the center. This gradually weakened the chroma.

Stimulus *G*.— A hue-change to yellow in the final stage. The chroma gradually grew less, the center remaining longer.

Stmulus *GY*.— Same as *G*, except that the change to *Y* was more marked.

The first part of the accompanying table shows the adaptation-times under the conditions described. The second part shows times obtained, still in direct sunlight, when a disc, composed as indicated in the table, was viewed against a large grey background. Observation in these circumstances was difficult, owing to the lack of a fixation-point.

ADAPTATION TIMES, IN SECONDS.

AVERAGE OF FIVE OBSERVATIONS

Obs.	Sh		B		Z		S		
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av. of Av.
R	198.2	6.5	192.6	10.5	190.8	9.4	194.8	5.0	194.1
O	89.6	3.9	89.4	6.9	91.8	5.4	93.8	1.4	91.1
Y	45.6	3.9	39.2	3.8	43.6	6.3	55.0	1.2	45.9
YG	49.2	7.1	53.2	5.8	50.4	6.5	52.4	7.4	51.3
G	52.0	6.8	54.6	9.5	49.6	2.9	56.6	3.5	50.6
BG	126.6	5.6	109.4	11.9	115.2	9.3	128.0	4.0	119.8
B	247.6	4.7	220.2	12.9	201.8	8.6	211.4	4.7	220.0
V	200.0	8.4	201.4	4.7	201.8	1.7	234.0	10.8	209.3
C	102.0	8.4	100.0	10.0	108.0	1.6	111.0	4.8	105.2

180° OF COLOR + 180° OF BLACK AND WHITE (BRIGHTNESS-VALUE)

R	93.8	7.0			104.0	8.6	96.8	12.6	98.2
O	42.0	5.2			45.5	3.2	45.2	4.2	44.2
Y	21.0	2.0			27.0	5.4	22.2	1.8	23.4
YG	25.4	4.7			29.0	2.1	26.0	4.8	26.8
G	26.0	7.8			23.5	6.6	21.6	4.3	23.7
BG	57.0	4.8			50.3	3.4	56.0	6.4	54.4
B	118.0	13.6			122.0	2.6	123.8	7.0	121.3
V	108.0	6.4			98.6	4.8	112.8	7.1	106.5
C	53.4	7.1			44.3	2.4	52.0	6.0	49.9

FOVEAL ADAPTATION AT MEDIUM INTENSITY
PRELIMINARY SERIES

We next sought to determine the experienced course of adaptation to color, at medium intensity, when only the fovea was stimulated by the color.

For stimuli, a circular disc, 100 cm. in diameter, was cut from each one of the 9 colors shown in the preliminary experiment. Every disc was mounted upon a gray background of 24 x 30 in. The background was free from any trace of color, and had a brightness-value of 140° white and 220° black. A frame on a table held the stimulus-card upright. The stimulus was placed at a distance of 225 cm. from the eye of the observer; the image of the stimulus-color fell within the macula.

The experimental work was done in a large light optics-room. Light window-shades were drawn over the windows, so that the room was mildly illuminated with a soft white light. The instructions to *O* were the same as in the preliminary experiment.

The quantitative results (times in sec.) are set forth in the following table. The averages are of 5 observations.

Obs.	Sh.		S.	
Color	Av.	M.V.	Av.	M.V.
R	137	7.5	178	8.1
O	140	6.5	140	8.4
Y	100	4.5	105	4.2
YG	88	4.5	95	7.0
G	80	5.5	110	6.2
BG	100	3.2	98	3.1
B	118	4.5	109	3.2
V	201	10.5	199	7.0
C	175	7.2	178	8.0

The reports of the observers may be summarized as follows:

R. The color began to lose in chroma very quickly. The outer edge of the circle paled out first, the paling gradually approaching the center. There was a little hue-change; the *R* faded slightly towards a very poor *O*. The resultant gray was a little lighter than the stimulus-color.

O. A thin gray film gradually thickened over the surface of the color. This caused the chroma to become poorer with the progress of adaptation. The hue changed slightly towards a *Y*, very poor in chroma.

Y. The hue faded away under the gray film until no color was perceptible. There was no hue-change.

YG. There was a hue-change to *Y*, very poor in chroma. The final stage was somewhat darker than the color.

G. The hue changed to *Y*, very poor in chroma. The gray film

very quickly obscured the surface of the color, only a greenish gray remained for some time at the last stage of adaptation. Slightly darker than the stimulus-color.

BG. There was no perceptible hue-change, only a fading out to a very pretty gray. The film began to come in from the edge of the disc, gradually approaching the center of the color. The gray field was a little darker than the stimulus-color.

V. The hue changed to *R* and then to *B*. The resultant gray was lighter than the stimulus-color.

B. There was no perceptible hue-change in this color. In every respect it faded out like the *BG*.

C. Gradually faded out to a *B*, very poor in chroma. This state persisted for some time. Then the field was almost of the same brightness as the stimulus-color.

It soon became evident that this experimental arrangement was unsatisfactory. The observers complained of eye-movement in almost every observation; and the difference in illumination from day to day influenced the adaptation-time considerably. Further, there is a marked difference between the adaptation-times of the two observers for certain colors, especially *R* and *G*. It was therefore necessary to secure better conditions of observation.

Part II.: Foveal Adaptation at Medium Intensity Controlled Series

Procedure.—The experimental work was done in a dark room. The apparatus and stimuli were those described in our account of the preliminary series. The illumination was produced by three 75-watt, 110-volt, Edison-Mazda daylight bulbs; it was equal to 212.4 c. p. at a distance of 2.5 m. from the stimulus-color and a distance of 1 m. above and a little in front of *O*. The current was supplied by a D. C. motor in the university service.

In order to avoid eye-movement, as far as possible, the following precautions were taken. (1) A time-recording apparatus was constructed, to register accurately the time of every change in visual sensation. This made it needless for *O* to speak during an observation. The recording apparatus consisted, first, of a kymograph, set to revolve once in 3 min. A double electrical contact-marker worked over the smoked drum. To one marker was attached in circuit a large seconds-pendulum, while the other marker was in circuit with a telegraph-key operated by *O* in reporting the sensory changes. (2) A comfortable head-rest was fastened to the table in front of the stimulus-frame. With this arrangement, *O* could hold the head in a constant position during an observation.

O adjusted the head-rest, covered one eye with a black velvet eye-shield, and made ready to fixate the stimulus when the

light was turned on. The instructions were as follows: "When the light is turned on, fixate the center of the colored disc, and hold it if possible without moving the eye. If the eye does shift from its fixation, bring it back to position as quickly as possible. When the light is turned on and you have fixated, press the telegraph-key five times in rapid succession. Every time the eye moves in any way during fixation, press the key once. For every color-change of any kind, press the key twice. When the color fades out, or when it reaches a point where it shows no further change, press the key again five times. You will be asked to give an introspective report of these changes at the end of every observation."

Results.—The results are given in summary for the different observers and stimuli.

Observer S. Stimulus *Y*.—An after-image came very quickly. The mist appeared from the outside, causing the color to become less saturated and much lighter. As the mist grew thicker, there was a tendency for it to fuse in the border before the center became solid; thus the center was the last part to disappear. The cloud was much lighter than the background.

Stimulus *BG*.—The after-image was noticed very quickly. Soon the cloud gathered over the surface of the color like a fog or mist. This caused the color to be much less saturated. For a little while I could see the color through the fog as if it were behind the mist. The cloud which covered the color was darker than the stimulus-color.

Stimulus *B*.—A soft white film began to form over the surface; at about the same time I observed the after-image; in fact, the after-image and the fog could be clearly recognized as one. The after-image was merely superimposed over the color, and appeared to stand out from the color. Where this haze fell on the outside edge of the stimulus-color and on the bordering background, it took on the nature of the after-image; where it was on the stimulus-color it was a beautiful white fog or mist. As this mist or fog gradually grew thicker, the color slowly lost in richness of chroma without any hue-change. The resulting gray was of the same shade as the background.

Stimulus *R*. The gray film was peculiar in this color. It came in from the outside. The color appeared very dark through it. The mist was not smooth, but rough, very much like hazy clouds on a summer afternoon. It never had a smooth texture, like the blue, yellow or orange. At times I noticed shooting phenomena from the outside, like smaller clouds suddenly moving across the surface of the disc.

Stimulus *V*.—The cloud suddenly came in from the outside, at about the same time as the after-image was noticed. It was very hazy at first, but slowly thickened to a very dense mist. The color did not seem to change in hue in any way, but the haze seemed to thicken and conceal it from view. The cloudy gray was somewhat darker than the background.

Stimulus *O*.—The after-image was seen very quickly, bordering the color almost all the way around; with this came a very peculiar cloud, which seemed to be a part of the after-image, extending over the color, hiding the color. For a while the view resembled the sun behind a luminous fog.

Stimulus *C.*— The color became almost a bright red behind the thin misty cloud, when the cloud would move a little to one side. The after-image and the cloud could be recognized as the same surface, floating over the surface as the eye moved. The cloud was smooth and not rough like the red. There were shooting phenomena as with the red.

Stimulus *G.*— The cloud appeared about the time the after-image was observed. When the haze became very thick, there was noticed a little yellow mixed with the misty surface. This was probably a hue-change of the stimulus-color. The cloud in the beginning seemed to move in from the outside, growing thicker towards the center.

Stimulus *GY.*— The cloud appeared as if in front of the stimulus-color in the form of a light hazy mist. It became thicker and thicker until the color was obscured. The color could be seen for a considerable time through the fog; as the mist grew thicker, the color appeared to lose in richness of chroma, until only a gray of about the same brightness as the background remained.

Observer Sh. Stimulus *Y.*— An after-image came very quickly, which extended all the way around the stimulus-disc. The after-image was at first very poor in chroma. Almost at the same time a whitish film began to form over the surface of the color. As it thickened, it appeared to be of the same brightness as the background. This gray surface began as a very thin sheet moving over the surface. At times it appeared as threads or filaments, stretching across the surface, as if they were in motion; these thickened into a solid uniform surface.

Stimulus *BG.*— There was a short interval of time before the after-image was seen on the border around the stimulus-color. The misty haze about the same time was observed to move inward over the surface of the color. The film soon thickened in texture so as to obscure the color.

Stimulus *B.*— The after-image began to show very quickly around the edge of the stimulus. The haze began to form almost as quickly, and thickened into a very white misty film. This stood out away from the color, and at no time did it appear to be a part of the stimulus-color. The color gradually lost in richness of chroma as the film thickened. When the cloud had taken on a filmy or sheet-like appearance, it was very easy to see that it was an extension of the after-image over the stimulus-color, as a misty white luminous fog. It looked very much like a thin sheet of tissue-paper spread over the color; the film at all times was in front of the color and not a part of the color. The whole adaptation-process for the blue was a fading out or disappearing of the color behind the fog.

Stimulus *R.*— The after-image appeared very quickly as a bluish-green border on one side of the disc. At first it was pale, or washed out. but it gradually increased in chroma until it was very rich. At the same time a haze began to form over the stimulus-color, which gradually grew thicker. The haze appeared to come from the outside of the disc and to settle in front of the color. This haze for the red was a rough, uneven mist or fog; not smooth in texture like that of the yellow or orange. The hue showed a remarkable change during the course of adaptation. It changed almost as soon as the haze began to form over the color into a red-orange, then to an orange of very poor chroma, and then to a very dirty, dingy brown. This brown was persistent for some time; finally it changed into a gray surface which was somewhat darker than the background.

Stimulus *V.*—A very pale, faded after-image soon appeared from

behind the stimulus-color, scarcely visible on the gray surface surrounding the stimulus. The cloud or fog seemed to float in from the side of the color towards the center; not so pronounced as with the red. The film at first was thin and more solid than with the red, but not so smooth in texture as with the blue or orange. (In three observations there was a slight hue-change towards blue, the red going out first. In two there was only a gradual fading out of the color into the fog).

Stimulus *O*.— The pale blue of the after-image, which was seen almost from the first, soon increased in chroma until it was a rich, well saturated blue. At the same time a beautiful hazy mist was gradually thickening over the color. This film appeared very much like the sun behind a thin fog. An interesting phenomenon occurred with a shift of the eye. There could always be observed two distinct discs in the visual field; the one was the stimulus-color, and the other was the film or fog in front of the color. These two discs, when not superimposed, the cloud upon the stimulus-color, always gave three parts of a circle. The part of the luminous fog which was upon the background showed a very rich after-image. The segment of the stimulus-color which was uncovered by the fog, caused by a shift of the eye, was a rich bright color, of the same hue as the stimulus-color; its brightness always depended upon the brightness-value of the background with reference to the brightness of the stimulus-color. The third portion was that part of the stimulus-color upon which a part of the cloud was superimposed. This was a beautiful gray, in which no color could be perceived in the final stage of adaptation.

Stimulus *C*.— The after-image was slow in appearing. The cloud effect was generally noticed as soon as the after-image. A change in brightness is always noticed as the filmy surface begins to form over the color. At this point the observer can always tell when adaptation is really progressing rapidly, because the color begins to pale out very rapidly. The luminous fog was always smooth, gradually thickening from a filmy surface to a veil very much like a thin sheet of paper spread over the surface of the color. This finally became so thick in texture that the color was obscured.

Stimulus *G*.— The cloudy mist began to form over the surface of the color by floating in from the outside towards the center of the disc. This movement and the after-image came at the same time. This haze grew thicker, which caused the color to become less and less saturated until only a gray surface was left. Generally there was a slight change in hue towards yellow.[17] The yellow would appear as if it were in the foggy veil which floated before the stimulus-color.

Stimulus *GY*.— The after-image began as a very pale, washed-out carmine. It extended all the way around the stimulus disc. With an increase in the richness of the chroma of the after-image also came a thickening of the hazy film which formed with the after-image. The cloud was apparently of the same brightness as the background. In nearly every case the green was observed to adapt out first, leaving a dirty, muddy surface which remained as a stubborn color for a considerable time.

Observer L. Stimulus *Y*.— Soon a film formed over the surface of the color, which did not appear uniform at first, but thicker in spots. The color began to lose in saturation from the first, changing its hue to a tan or very light unsaturated yellow. The haze finally grew so thick that the color was obscured from view.

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[17] This was no doubt due to the fact that the Milton-Bradley *G* contains a slight amount of *Y*.

Stimulus *BG*.—The after-image and the cloud-effect appeared very quickly and at almost the same instant. The fog was very light and fluffy. It stood out in front of the color. The hue of the color gradually faded away behind this white mist as the fog became thicker. The final stage of adaptation gave a gray surface equal in brightness to the background.

Stimulus *B*.—The after-image was quickly observed; at about the same time the misty cloud spread quickly over the surface of the color. The fog was almost pure white and appeared to fit the stimulus-color. It seemed to stand out away from the color. This mist gradually thickened into a thick cloud which hid the color of the disc.

Stimulus *R*.—The cloud floated in over the surface of the color from the outer edge of the disc. It was not smooth in texture, but somewhat rough and uneven. The red disc changed at first into a kind of orange, then to a yellowish color of very poor chroma, and finally faded out to a dark gray.

Stimulus *V*.—The after-image appeared very bright from the beginning. The misty haze is somewhat darker than the background. It gradually thickened over the color. It is difficult to tell the final gray, when adaptation is complete, from the violet when it is very poorly saturated.

Stimulus *O*.—The haze over the color and the after-image around the stimulus appeared at the same time. The cloud thickened and obscured the color from view. For some time the color could be seen as if shining through the fog. The final gray was of the same brightness as the background.

Stimulus *C*.—The after-image was at first dim and slow in appearing. The cloud was very thin at first and somewhat darker than the background. The color gradually paled away in the fog.

Stimulus *G*.—A very weak after-image appeared at first, which gradually increased in chroma. A misty cloud came in from the outside of the color towards the center. This haze increased in thickness until it obscured the stimulus-color. It is of the same brightness as the background.

Stimulus *GY*.—Cloud-effect appeared very quickly. This caused the color to become very much less saturated, as if a thin sheet of white paper had been suddenly spread over the surface. The mist thickened so as completely to hide the color. The final gray is the same as the background.

ADAPTATION TIMES IN SECONDS

AVERAGE OF FIVE OBSERVATIONS

Obs.	S		SH		L		
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av. of Av.
R	84.8	7.0	91.4	7.9	98.0	6.4	91.4
O	88.2	7.4	92.2	5.4	92.4	11.1	90.1
Y	97.6	5.9	90.8	3.8	98.8	7.4	95.7
YG	62.0	6.4	63.2	3.4	58.4	7.5	61.2
G	55.2	1.3	58.4	3.5	55.0	3.2	56.2
BG	42.0	5.2	39.8	4.2	39.2	3.0	40.3
B	125.0	7.2	107.2	12.6	128.4	4.9	120.2
V	82.4	2.4	84.0	3.2	78.0	5.6	81.3
C	73.2	6.2	73.4	7.3	75.4	6.9	74.0

The curve for the times of adaptation in sunlight differs considerably from the curve for the same colors at medium intensity. *R* is relatively shorter at medium intensity, while *B* is slightly longer than *B* in sunlight. On the other hand, the *Y* and *G* regions of the two curves take almost opposite positions. There is not only a rapid rise in the *Y* region for medium intensity, but there is a shift of the shortest time to the *BG* region. This difference might evidently be due to a difference in the chroma of the *Y* and *G* regions, caused by the two sources of illumination.[18] We therefore determined the chromatic limen of observer Sh for every color under both sources of illumination.

Procedure.—A color-mixer was placed in the window, on the table used in the preliminary experiment, so that the discs should revolve in a horizontal position. The disc was made up of a black-velvet paper, a good white, and a colored paper. The relative proportion of the brightness-value of the color was kept constant by varying the black and white discs as the color component was increased or decreased. The method of limits was employed (five series). *O* did not see the disc until it was in motion, and then only long enough to make a judgment. The experiment was repeated in the dark-room with the illumination used for medium intensity, and also in diffused daylight.

The *results* are shown in the following table:

CHROMATIC LIMENS

COLOR	SUNLIGHT		DARK ROOM ILLUMINATION		DIFFUSED DAYLIGHT	
	Av.	M.V.	Av.	M.V.	Av.	M.V.
R	18.0	3.6	22.0	6.4	19.3	1.6
O	34.0	3.8	20.0	6.2	8.0	1.3
Y	55.0	6.1	18.0	3.8	19.7	2.5
YG	46.0	4.3	28.0	5.2	23.0	3.6
G	37.0	4.3	32.0	4.3	22.2	2.3
BG	30.0	2.2	40.0	5.4	19.0	1.6
B	12.0	3.2	12.0	1.1	11.0	1.6
V	22.0	4.6	30.0	3.6	33.0	6.3
C	26.0	6.3	25.0	4.1	13.3	3.1

[18] The gray background upon which the stimulus-color was mounted, and which tested free from color under the daylight electric-bulb illumination, gave the following composition when placed in direct sunlight illumination: Black 215°, White 125°, B 12°, and G 8°.

There can be no doubt as to the effect of chroma on the time of adaptation in direct sunlight, and in illumination used in the dark-room experiment. The proportion, through the whole series of nine colors, runs in the right direction. Wherever the adaptation-time is long, the chromatic limen is small, and conversely. For diffused daylight, there is a slight variation; *R* and *V* are out of proportion. This exception may be due to the fact that *R* and *V* are the dark colors; since the illumination was rather weak, the limen would tend to be high. For all the lighter colors there is a close approximation to the law stated above, that where the time is high the limen is low. We shall later find that the same thing holds for spectral colors; chroma is the principal factor which influences the time of adaptation, with a possibility of intensity as a minor factor.

Part III.: Pure Spectral Colors

We proceed to describe the course of foveal adaptation to pure spectral colors, and to compare the behavior of these colors with that of surface-colors. We were careful to present the spectral color to *O* under controlled conditions. The wave-length of the spectral band producing the stimulus was determined before every observation; the intensity of the light producing the spectral color was always known; and the amount of light entering the eye was kept constant by the use of an artificial pupil.

Procedure.—The apparatus consisted of a remodeled spectrophotometer.[10] In addition to the original equipment of the apparatus, we inserted a field in front of the lens, so that any desired width of spectral band could be obtained in the form of a circular disc of color. An artificial pupil, 2.5 mm. in diameter, was placed 14 cm. in front of the stimulus field, and 2 mm. from the eye. A small projection-lantern, burning a 200 w. 115 v. Mazda nitrogen-filled daylight bulb, illuminated the spectroscope to produce the spectral colors. Before the lantern was placed a plate of thin and very finely ground glass to cut out the image of the filament in the light-bulb. The amount of light that entered the collimator of the spectroscope was 61.4 c. p.

The observations were made in the dark-room. All the light for illuminating the spectroscope was thoroughly concealed, so that only the color through the artificial pupil could be observed. A head-rest was set in front of the spectro-

[10] Manufactured by the Cambridge Scientific Instrument Co.

scope.[20] All changes in color-sensation were recorded in seconds by means of the apparatus described in Part II. *O* was given 15 min. of dark-adaptation before beginning the experimental work, and a period of 5 min. was allowed for rest between observations. Since monocular vision was employed, and the eyes were alternated from observation to observation, there was a period of at least 10 min. before an eye was restimulated.

For stimuli, circular fields were cut from the spectral band having the following wave-lengths; *R* 740-770 $\mu\mu$, *Y* 545-575 $\mu\mu$, *BG* 495-525 $\mu\mu$, *B* 440-470 $\mu\mu$, *V* 405-435 $\mu\mu$. The field was 6 mm. in diameter, and was just large enough to fall well within the macula lutea. The constancy of the light could be tested at any moment by means of a switch which turned the current through voltmeter and ammeter.

The *results* appear in the following table, as averages of five observations.

ADAPTATION TIMES IN SEC.

OBS.	SH.		S		Z		F		L		Av. of Av.
Color	Av.	Mv.	Av.	Mv.	Av.	Mv.	Av.	Mv.	Av.	Mv.	
R[21]	102	4	103	6	110	9	98	7	109	13	104.4
Y	166	10	164	10	180	21	Failed		166	7	169.0
BG	119	9	126	10	132	6	164	16	117	3	131.6
B	103	6	104	2	120	4	121	10	90	6	107.6
V	69	8	69	9	78	4	90	6	77	6	76.6

Observer F failed to get complete adaptation for *Y* at this intensity. In explanation we can only state that she had had less practice than the other *O*'s when she began the observations. All the others reported complete adaptation for all stimuli used.

The curve of the adaptation-times rises from *R*, reaches its maximum at *Y*, and gradually descends to *V*. This is almost an exact figure of the brightness-curve for spectral colors.[22]

[20] It was found at the beginning that any movement, even that of winking the eye, influenced the time of adaptation. The head-rest enabled the observer to hold the head in one position, leaving only steady fixation to be attended to. After a little practice with the artificial pupil, a good observer has no trouble in holding an observation period, often without even winking the eye.

[21] A psychological *R* was made up from gelatins. It was composed of *R* and *B* gelatins so combined that no hue-change was observed during the process of adaptation. The time of adaptation was: Sh 100, Mv. 3.2; S 104.2, Mv. 2.1; Z 104.9, Mv. 6.4; L 98.9, Mv. 11.2. F did not observe in this part of the series.

[22] W. de W. Abney, *Researches in Color Vision*, 78-111.

Hence we may have recourse to two factors, to explain the difference in the curves for surface-colors and for pure spectral colors: brightness and chroma. To test the first factor, we equated all the colors for brightness-value and then repeated the series, with the following result:

ADAPTATION TIMES IN SEC.

COLOR	SH.		S		L	
	Av.	Mv.	Av.	Mv.	Av.	Mv.
R	96	5.4	82	4.5	105	3.1
Y	115	6.6	120	3.0	130	6.9
BG	97	5.5	100	6.7	96	4.3
B	59	3.1	68	6.5	65	1.1
V	68	3.1	52	5.0	68	2.3

It is clear that an equation for brightness does not altogether remove our difficulty. To test the second factor, namely, the possible effect of chroma, we arranged a series of experiments as follows. Three color-fields were used: 4.5 mm., 3 mm., and 2 mm. in diameter. Since *V* has the lowest adaptation-time, a stimulus of this wave-length and brightness was set up on a second spectroscope. White light was added to another color in the spectrophotometer until the observer judged the two colors equal in chroma. When this point was reached, the observer was given 15 min. of dark-adaptation, and then observations were made with the three fields; the same colors were employed as in the first part of this experiment. The following are the averages for 10 observations by every observer on every field:

3MM. FIELD

OBS.	SH.		S		Z		F		L	
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
R	47.2	5.4	63.4	3.3	48.0	6.0	62.4	4.8	67.0	9.2
Y	47.2	3.4	66.4	2.3	48.2	5.1	62.8	9.6	63.8	5.4
BG	47.2	2.6	65.2	7.4	46.0	4.4	60.5	3.1	74.2	4.8
B	41.8	1.8	57.8	5.4	45.6	3.4	61.9	3.1	70.0	2.8
V	44.4	2.4	63.0	4.4	46.2	4.2	62.6	8.2	67.8	3.4

4.5MM. FIELD

R	64.4 6.7	57.8 3.5	59.2 4.6	64.4 5.3	55.0 9.6
Y	46.0 4.4	75.8 5.0	46.0 4.0	50.2 1.9	74.2 6.5
BG	47.4 6.8	73.4 4.5	43.6 3.7	89.0 1.2	80.8 3.8
B	54.6 6.8	63.4 8.8	46.6 7.1	71.6 7.9	67.0 6.4
V	46.0 3.2	63.8 2.6	44.8 1.8	106.0 8.8	64.4 8.6

2MM. FIELD

R	50.6 5.7	62.0 4.8	46.2 3.8	55.1 6.3	64.2 10.6
Y	62.6 1.9	59.4 8.3	53.8 3.8	61.2 6.8	64.8 7.4
BG	47.8 7.0	56.6 10.0	50.6 5.9	70.0 10.2	65.0 5.2
B	52.8 9.8	69.2 3.0	55.0 4.4	51.1 7.1	63.8 5.6
V	57.0 2.0	58.6 8.5	53.4 4.7	52.4 6.4	67.2 14.1

The introspective reports may be summarized as follows:

Observer Z. *Y.*—After about 30 sec. I observed a thin filmy cloud which began to form on the rim of the circle. The cloud was darker than the *Y*, and contained some color. This film became thicker and darker until there was no color left. The *Y* under the film always became brighter, paler, and gradually took on a washed-out appearance.

R.—The red began to get lighter. The hue began soon to change to a very good orange. This began about the time a thin film was forming over the color. The filmy surface gradually grew darker and lost color, until a good gray surface covered the color. At the same time the hue changed from *O* to *Y*, and then finally to a gray.

G.—The green began to fade out to a *Y* as soon as it began to lose chroma. The loss of chroma began along the border of the colored field. I could recognize the after-image, when the eye moved, as being a portion of the filmy surface over the color. The film was a soft white cloud. The *Y* then faded out to a very good gray.

B.—The whole surface began to brighten up together with a reduction in chroma. There was no change in hue. The chroma gradually faded out to a good light gray. This process was like a cloud gradually thickening so as to obscure a color beneath it.

V.—The *V* began to lose chroma very quickly; at the same time there was a change in hue; first the red faded very quickly to a certain degree, then the blue, and finally both faded out together. This loss of chroma was again in the form of a white film collecting over the surface of the color. Began on the border of the field, and then spread to the center, all the time growing thicker until the color was obscured.

Observer S. *Y.*—There was no change in hue. In the beginning of observation there appeared a light ring around the edge of the stimulus-color due to contrast. The color gradually paled out to a good light gray surface.

R.—The red went immediately to a very pretty orange, and from this to a yellow. A dark gray cloud appeared in the center of the disc. This gradually spread out over the surface of the color, and covered the whole disc.

G.— There was no hue-change; only paled out to a good gray surface. The final stage was almost a white. The cloud first appeared in the center of the disc and spread over the whole surface of the stimulus-color.

B.— The blue did not change in hue, but lost in chroma, until a very pretty gray surface remained. The color gradually grew lighter as it lost in chroma. There appeared a gray cloud in the center of the disc which spread over the entire surface.

V.— The hue of the violet changed, first to blue, losing the red; then the bluish-violet lost in chroma, until a very dark gray surface remained. The cloud appeared in the center of the disc and spread over the surface of the color.

Observer Sh. *Y.*— A whitish film began immediately to collect over the surface of the stimulus-color. It appeared to be a little brighter than the color. The film gradually grew thicker until it obscured all the color, leaving only a light gray surface. There was no perceptible hue change.

R.—The red went through a color-change, first to orange, then through *O* to yellow, and finally to a dark gray. A film was observed forming over the surface of the color at the same time it began to change in color. The film began to form in the center of the stimulus-disc, and gradually spread outward, covering the whole disc.

G.— There was a change in brightness at the beginning. A bright ring was noticed around the edge of the field. This extended inward towards the center very quickly. Then a light film began to form over the surface of the color. This gradually grew thicker until all color was obscured. There was no hue-change.

B—There was a decided change in brightness. A light film formed immediately over the color. This grew thicker until all color was obscured. The resulting gray is almost a white.

V.— There was a change in hue towards the blue. The red faded out first; at the same time there was a loss in chroma, by a thin cloud forming over the surface. A very dark gray finally formed over the color.

Observer L. *Y.*—The color became a little lighter, and lost in chroma until it became a light gray. There was no hue-change.

R.— The *R* changed to *R-O, O, Y,* and finally to a dark gray.

G.— There was no hue-change. A cloudy mist began to form in the center of the circular field, and spread over the entire surface, which decreased the color in chroma until it was completely faded out.

B.— There was a gradual loss in chroma; no hue-change. A cloud-mist formed immediately over the surface of the color, which gradually thickened until no color was noticed. The final stage was almost white.

V.— There was a change in hue to a violet-blue color. The red almost faded out first; then the color was poor in chroma. The final stage was a very dark gray.

Observer F. *Y.*— The color grew paler by losing in chroma. There was no hue-change.

R.— There was decided hue-change in this color. It went to *O*, then to a very poor *YO*, which reminded me of a dirty *Y*. Finally lost in chroma, until a gray resulted which was darker than the color.

G.— The *G* lost in chroma by paling away. No hue-change.

B.—Same as the green.

V.—The *V* changed to *R* and then to *B,* all the time losing in chroma.

Since a chroma-match between two colors that differ in hue is only a relative matter, there will always be some variation. So far, therefore, as time of adaptation is concerned, we have every reason to be satisfied with our results. The averages for the 3 mm. field are, it is true, more uniform than those for the other two; the 4.5 mm. and the 2 mm. fields show slight fluctuations. But no variation is as great as with the brightness-equation.

From the introspective reports we find that there is no change in hue except for *R* and *V*. All the other colors which we used faded away steadily to a gray field. The adaptation-process makes its appearance in the form of a film or mist over the color. This causes the color to become less and less saturated, exactly as was the case with the surface colors. When there is a shift of the eye from fixation, *O* always reports three continuous portions of the field: a crescent of after-image; on the opposite side to this, a crescent of stimulus-color, very rich in chroma; and between the two crescents a white or gray surface over the stimulus-color.

ADAPTATION AT HIGH INTENSITY

It was found, in the first series with spectral colors, that foveal adaptation would not proceed to a neutral gray when the light that entered the collimator of the spectroscope was raised to more than 60 c. p. At the same time three important facts were observed in regard to foveal adaptation at high intensity. (1) All our records show that there is a tendency for eye-movement to increase very rapidly at the end of a fixation-period, in some cases as much as 4 or 5 times the amount at the beginning of a period. (2) Our *O*'s report that adaptation is always very rapid at the beginning of a period, no matter how high the intensity of illumination may be, provided it does not produce a blinding glare.[23] (3) There is always a good deal of pain in fixating a bright colored light, even with foveal fixation, when the intensity passes much beyond 100 c. p. This pain may be noticed even as long as two or three days after the observation. It appears, then, that when a certain intensity is reached the eye moves so frequently, during the latter part of the period, that the process of adaptation can not be completed. The pain is probably caused by the excessive contraction of the pupil in response to the reflex set up by the very intense beam of light striking the fovea through the artificial pupil. In the following experiment we tried to overcome these difficulties, and to study the process of adaptation at high intensities.

[23] See P. G. Nutting, *Trans. Illum. Eng. Soc.*, xi, 1916, 943.

Procedure.—The exposure-apparatus was a projection-lantern which burned a carbon 13 mm. in diameter, carrying a 30 amp. (125 v.) D. C. current. The lantern was mounted with a projection-lens of 2 in. diameter. In front of the projection-lens was placed a short-focus lens to bring all the rays of light to a pencil-focus. The actual amount of light focussed by the lens was about 6,200 c. p. The head of the observer was held in a steady position by means of a head-rest. The focus of light was adjusted to fall within the eye. Colored gelatin-filters (Eastman Kodak Co.) of known wavelength and transmission-power were used for stimuli; a filter of any desired color could be immediately fitted into one side of a pair of goggles, made to hold the filter securely. In the other side of the goggles was fitted a dark piece of cardboard, to exclude all light from the unstimulated eye. A foveal comparison-light was set up in such manner that any spectral color, from the intensity afforded by the lantern to the lowest possible intensity, could be obtained immediately by shifting the lever of a rheostat. We measured the actual intensity of the light that entered the eye through the filters by the following method. A 500 watt Mazda lamp was screened to match daylight (acetylene flame and No. 79 filter) and was set up at one end of the photometer bench This procedure was adopted because the photometry of a carbon arc for this purpose is not as precise as the photometry of an incandescent lamp. A flicker photometer and a 20 c. p. comparison lamp were set up at the other end. When necessary, neutral-tint filters were used to reduce intensities.

For determining the transmission, a setting was made with the filter screening the 500 watt lamp, and then a setting was made without the filter; the ratio of these two readings on the candle-power scale gave the transmission. The following is an average of several trials:

Color	Filter No.	Transmission	x C.P. of Lantern=About	
R	70	7.6%	471.2	c.p.
Y	73	35.9%	2225.8	"
BG	75	18.6%	1153.2	"
B	48	19.5%	1209.0	"
V	76	1.6%	99.2	"

All work was done in the dark-room. *O* adjusted the filter to the eye, placed his head in the head-rest, and awaited the stimulus-light. The instructions were: "Fixate the bright circle of light until all color disappears. When you no longer see any color, remove the goggles, fixate the spectroscope, and describe what you observe." The following are the average adaptation-times in sec. for four observers:

	Sh	92	The hue quickly changed to *O*, then to *Y*, very poor in chroma. The *Y* persisted for some time. Finally all hue disappeared.
R	Z	109	
	S	120	
	F	100	The foveal comparison-color was pure gray, much lighter than the stimulus-color. All intensities were gray.
	Av.	105.3	
	Sh	165	There was no change in hue. The color gradually faded away.
Y	Z	177	
	S	150	
	F	175	The comparison light at all intensities was almost white; no trace of color.
	Av.	166.8	
	Sh	120	No change in hue.
BG	Z	112	
	S	98	Very light gray. No color at any intensity.
	F	130	
	Av.	120	
	Sh	240	No change in hue.
	Z	182	
B	S	155	Very light gray for all intensities.
	F	148	
	Av.	181.3	
	Sh	200	There was a change in hue; the *R* apparently weakened first; then there was a noticeable decrease in chroma for the *B*. The *R* disappeared, leaving only a very weak unsaturated *B*, which slowly faded into a dark gray.
V	Z	183	
	S	185	
	F	220	
	Av.	197.0	The foveal comparison light showed no trace of color for any intensity.

We were able to get complete foveal adaptation for all *O*'s with these intensities, as for all lower intensities. After a long fixation, beyond the point where adaptation was complete, we were able even to raise the comparison-light somewhat higher than the standard, and still had complete adaptation in the fovea. However, long fixation with a bright light is very painful; the pain even persists for two or three days as an ache or soreness within the eye.

It might be objected that the blinding glare of the positive after-image here influenced the judgment. We were, however, careful to test this factor before finally deciding upon the technique for the series. If the arc was fixated without the filter, there always resulted a blinding glare. When the foveal

comparison-light was fixated in this condition, there was only a slight change in brightness of the color, which immediately passed away. This was not true when a stimulus-color was fixated. The foveal comparison-light was always colorless, and so remained. It must, however, be small enough to fall within the foveal cup, and must not extend to the periphery of the macula.[24] Otherwise a more pronounced effect will be noted from glare.

Adaptation at high intensity is difficult to describe; but so far as we were able to observe the same general process takes place that has been observed throughout the earlier experiments of this study.

Conclusions

In general adaptation to direct sunlight, as well as under other conditions of adaptation, both to surface and to spectral colors, the fovea has a longer adaptation-time for color than any other part of the retina. This law aparently holds for high intensities as well as for medium and low intensities.

Chroma is the principal determinant of the rate of adaptation. Intensity is a second factor. The effect of both chroma and intensity has been observed with surface colors as well as with spectral colors.

The rate of adaptation is always faster at the beginning of a fixation-period, gradually decreasing, and becoming very slow at the end. The rate at the beginning also depends upon the chroma; the richer the chroma, the faster is the initial rate of adaptation. It gradually becomes slower later, and the whole process lasts longer than for a color poorer in chroma.

There is no difference in the experienced course of adaptation between a surface color and a pure spectral color. A color, under the process of adaptation, very quickly begins to decrease in chroma. This decrease is observed in the form of a light film which spreads over the color. The film continually thickens, until the color is obscured under the fog. The appearance of a color after adaptation corresponds in general to a mixture of that color with its complement. This relation was observed in nearly every observation. If the eye moved slightly from fixation, there were always reported three parts in the field: a crescent of the after-image, an opposed crescent of the stimulus-color, and an intermediate neutral gray surface.

Under suitable conditions, adaptation in direct vision can be carried to completeness at any physiological intensity.

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[24] J. von Kries, *Z. f. Psych. u. Phys. d. Sinnesorg.*, xv, 1897, 327.

THE FOSTER-CHILD FANTASY

By Edmund S. Conklin of the University of Oregon

This study was designed to test the validity of the generalization made by the psycho-analysts that most or all children experience the foster-child fantasy, and to determine some further facts concerning the phenomenon. As a stage in the development of the "family romance" it is said that all or nearly all children conceive the notion that they are the children of much greater parentage. If this is so, especially as some think that it exerts much influence upon the conduct of the child, it is of great interest to students of childhood and adolescence.

The generalization seems to be well on the way toward acceptation. Abraham apparently considers it universal and sufficiently well demonstrated (1. pages 40-41). Adler refers to it as a not infrequent occurrence (2. page 206). Hall in his interpretation of Jesus' concept of divine parentage treats the foster-child fantasy as a very common experience of childhood (3. pages 251-2). Jelliffe is more daring and states that the family romance is a "universal phenomenon" (4. page 54), although he adds that any stage, and this could of course apply to the foster-child stage, may be "much abbreviated." White treats it as established in his introduction to psychoanalytic psychology (6.). Many Freudian writers, however, refer to or use the concept of the family romance with an air of certainty but without specific mention of the foster-child fantasy. Possibly they assume its existence as a part of the romance. Rank's study of the myth of the birth of the hero is most frequently given as reference (5.). Both Jelliffe and White quote Rank's presentation at length. And Rank presents the matter with apparent confidence in the conclusiveness of its demonstration. He also says that psycho-analyses are the only means of determining such traits (5. page 63).

Acceptance of the establishment of the foster-child fantasy as a common trait of childhood has hitherto depended upon the validity of conclusions from psychoanalyses, of psychoneurotic adults mostly, some of whom died long prior to the time of the analysis. That Rank's presentation so often quoted occurs in the course of a study of mythology should

not be permitted to lead to the supposition that it is an induction from his myth data. He himself says that it is a summary of the traits of childhood discovered by Freud "from his experience with the psychology of the neuroses" (5. page 63). Whatever may be thought of the validity of generalization from such data, it must be evident that here is an opportunity for checking the results of psycho-analytic work by use of another psychological tool, the questionnaire. In fact it is for just such work as this that the questionnaire is best suited, for a census of certain human experiences. If it is true that most children wonder whether "they can really be the offspring of their prosaic parents" and develop notions about their real parentage, then it should not have entirely disappeared from voluntary recall by mid- or even later adolescent years. Hence questions presented to adolescents under circumstances calculated to arouse a sober attitude of willingness to cooperate should produce other and valuable evidence concerning the frequency and nature of the foster-child fantasy and serve at the same time as a check upon the generalization made from psycho-analyses. Such is the nature of the study here presented.

With such a purpose, the questionnaire could be very simple and direct. After many preliminary experiments it was finally used in the following form:

It has been observed that some people during childhood or youth acquire the notion that their parents are merely foster parents. The purpose of this questionnaire is to determine how common such an experience is and some facts about it.

Your sincere effort to answer the following questions accurately will be appreciated.

Negative answers if true are just as valuable as positive.

Write answers in the space following each question.

1. Age at present.
2. Sex.
3. Did you ever have the experience of day dreaming or believing that you were an adopted or foster-child, that your supposed parents were not your real parents? (Answer yes or no).
4. Was it merely a day dream or did you believe it to be true?
5. Who or what did you think that you were?
6. If you can remember what caused such thoughts, please state it as clearly as you can.
7. About how old were you at the time?
8. Did these ideas lead you to any special acts or changes of conduct? If so, please state what.

9. If you believed yourself an adopted or foster-child, about how long did the belief last?

10. Has the belief disappeared? Tell why it disappeared if you can.

11. State below any remarks or information which may be of interest concerning the matter.

This was answered by students in scientific departments of the University of Oregon and by students in the two upper classes of eight different high schools in the same state. The college students wrote under the supervision of the writer or one of his colleagues in psychology. In the high schools they were written under the supervision of the superintendent or principal, people experienced in the handling of students and here selected for their interest in research. Several of them had formerly been students in the writer's department. In order to insure as nearly perfect uniformity in the presentation of the questionnaire as possible, printed directions were supplied to all who assisted in its presentation. Care was taken to make certain that the students understood that a serious purpose existed for the questions, which otherwise might have provoked frivolity, and by this means serious answers were obtained. Out of more than nine hundred returns only five bore any indication of insincerity. However otherwise this study may be attacked the author is confident that insincerity in its data is not one of its weaknesses.

Nine hundred and twenty one returns came to the writer. Of this number five were rejected because of apparent insincerity, ten because of failure to answer some important question such as age or sex, and two more were dropped because they were actual instances of foster children. Thus the total number of returns upon which this study is based became 904. Of these 640 were from the two upper classes in the high schools and the remaining 264 were college students, mostly freshmen and sophomores. Their distribution by age and sex follows:—

Age Distribution

Age	14	15	16	17	18	19	20	21	22	23	24	25	over 25	Total
Male	2	11	38	86	68	56	44	21	13	5	5	2	7	358 or 39.6%.
Female	5	10	70	168	134	69	43	17	11	6	4	3	6	546 or 60.4%
Total	7	21	108	254	202	125	87	38	24	11	9	5	13	904

Socially this group might best be characterized as homogeneously American. Students of foreign born parents were few and students of foreign birth rare. They came as a

rule from homes of moderate to considerable culture, identified chiefly with commercial, agricultural or professional pursuits. Extreme poverty or utter illiteracy would rarely be found.

Frequency of the Fantasy

Question No. 3 was designed to discover the frequency of recallability of the fantasy and was worded to include with affirmative answers all degrees of belief or unbelief. It was answered by all of the 904 students participating in the study. The distribution of the answers was as follows, affirmative indicating recall of some experience with the fantasy and negative no immediate recall:—

	Male	Female	Total
Affirmative	87 or 24.3%	171 or 31.4%	258 or 28.5%
Negative	271 or 75.7%	375 or 68.6%	646 or 71.5%
Total	358	546	904

Age Distribution of Answers to Question No. 3

Age	14	15	16	17	18	19	20	21	22	23	24	25	over 25	Total
Affirmative														
Male	1	4	8	27	13	8	13	4	3	1	3	—	2	87
Female	1	2	22	50	41	20	14	8	2	5	2	2	2	171
Total affirmative	2	6	30	77	54	28	27	12	5	6	5	2	4	258
Negative														
Male	1	7	30	59	55	48	31	17	10	4	2	2	5	271
Female	4	8	48	118	93	49	29	9	9	1	2	1	4	375
Total negative	5	15	78	177	148	97	60	26	19	5	4	3	9	646
Grand total	7	21	108	254	202	125	87	38	24	11	9	5	13	904

It will be observed that the median for the affirmative group is the same as for the negative group. The likeness of these distributions indicates that there is no significant change in the frequency of recallability with age, that for the mid-adolescent years at least there is no rapid suppression. The older ones recall the experience as frequently as the younger. If this phenomenon continues on into the higher ages it would be out of harmony with the psychoanalysts' assertions concerning the disappearance of childhood fantasies from voluntary recall, but the data here for the upper ages is not sufficient for generalization. It merely suggests an alluring opportunity for further research. That 28%, however, of this group of more than nine hundred young people can immediately recall having experienced the fantasy does lend considerable support to the assertions of the psychoanalysts concerning its frequency.

Attitude Toward the Fantasy

Preliminary experimentation and inquiry revealed that some people could recall definitely having experience with the fantasy but merely as a day dream while others had actually believed themselves foster-children. For this reason question No. 4 was designed to bring out the frequency of the two groups, but it did rather more than was expected. It led to answers which revealed many gradations of belief. Some would not even admit that it was a day dream and used expressions of which a few follow:—"I merely wondered if I might belong some place else," "Just a casual thought," "Did not believe it to be true but thought of possibilities if it was," "Merely wondered," "Merely a random thought not developed to any degree or thought of later," "Just pretending," "Just a thought that I knew at the time wasn't so," "I liked to pretend my parents were foster parents." For the lack of a better term such answers were classified below under the heading, Casual Thought. Of those who answered that they believed their fantasy to be true some added qualifying phrases which also indicate gradations on the scale of belief but at the other end of the scale from the samples just given. Here follow some of the phrases in the belief group;—"I believed that it might be true," "I perhaps believed it true," "Day dream at first later believed it to be so." "Half believed it at the time," "I believed it or rather hoped it to be true," "Perhaps I did not fully believe I had foster parents but I doubted that my parents were truly mine." But most of the answers in this group leave no room for doubt for they are such phrases as "I believed it" or "I thought it to be true." The day dream group also indicates occasionally such gradations of belief as indicated by the following quotations;—"A day dream but at times was uncertain," "Merely a day dream but thought on it a good deal," "More of a day dream rather wishing I might be some one else's child," ". . . . wondering how it would feel to be a foster child," "Day dream but I nearly believed it to be true," "It was dreaming I suppose and yet I often thought that perhaps I had had a different father," "Day dream afraid it might be." Below are the frequencies of the answers in tabular form.

Frequency of Belief in the Fantasy
(Answers to Question No. 4)

	Male	Female	Total
Belief	17 (19.5%)	54 (31.6%)	71 (25.4%)
Day dream	58	101	159
Casual thought	12	15	27
No answer	—	1	1
Totals	87	171	258

From this it will be seen that all but one of those who answered question No. 3 affirmatively gave some indication of their attitude toward the fantasy. Twenty five per cent of all who could recall the experience recall it as being believed at the time. More than 19% of the males believed it and more than 31% of the females. Of all the 904 answering the questionnaire 7.8% recalled experiences of the fantasy and believed it, 4.5% of all the males had it and believed it, and so did 10% of all females.

Forms of the Fantasy

(Answers to Question No. 5)

Answers to this question indicated several degrees of development of the fantasy. Many returns merely said "adopted child" or "Never thought that far" or other words with like meaning. Such answers are grouped in the table below under the first heading as they seem to represent an incipient or undeveloped stage of the fantasy. Then there are those, of considerable number, who thought themselves foundlings of insignificant inheritance or destitute orphans who had been taken out of pity from an asylum. A few of these answered with the single word, orphan; but most gave some qualifying phrase or word which indicated socially inferior origin. These are all grouped below under the second head. A small number very clearly stated that they thought themselves children of parentage of a similar social status (third heading in the table). Another considerable group had developed ideas of greatness. Kings, princes, great wealth, great beauty, great intellectual powers,— all these figured in the concepts of their supposed true parentage. A few thought of themselves as strange, mysterious or supernatural beings, using these very adjectives on their returns, and they are so described in the table. It may be worth while to add that these few did not all come from any one community but from different parts of the state, thereby eliminating the possibility of collusion. Then there were those who recalled the fluctuating nature of the fantasy and report themselves as thinking of now one and now another type of person. Under Miscellaneous and Indefinite are presented those who gave such answers as "Kidnapped," "Traded by mistake," etc. making them otherwise unclassifiable. The frequency of occurrence of each group is indicated below as well as the percentage which the total of that group is of the entire number answering this question.

	Male	Female	Total
Incipient stage of fantasy (No clear concept)	47	75	122 or 54%
Orphan or foundling	7	26	33 or 15%
Parentage of similar social status	2	5	7 or 3%
Child of great parentage (Great wealth 20, royalty or nobility 11, miscellaneous 9)	8	32	40 or 18%
Strange, mysterious or supernatural being	4	1	5 or 2%
Many different characters	4	6	10 or 4%
Miscellaneous and indefinite	2	5	7 or 3%
Totals	74	150	224

Of the two hundred and fifty eight people whose answers to Question No. 3 indicated experience of the fantasy, two hundred and twenty-four or 87% answered this question. It was answered by 85% of the males and 88% of the females who answered No. 3 affirmatively.

Causes Offered for the Fantasy

The following table summarizes the answers to question No. 6 and in most instances the items are in the words of the questionnaire returns. The percentage figures indicate the per cent which the accompanying figure is of the total number of that class answering this question. (Totals here cannot be checked with totals in other tables because here some give more than one answer).

	Male	Female	Total
1. Suggestion: Books, stories, etc., and the hearing and seeing of actual instances of foster parentage.	30 (43%)	66 (46%)	96 or 45%
2. Dissatisfaction: Limitations of home, longing for wealth, discontent.	1 (1%)	9 (6%)	10 or 5%
3. Mistreatment (Actual or supposed): Harshness, partiality, teasing, fault finding, inconsistent treatment, desire for affection, lack of respect for parents.	19 (27%)	34 (24%)	53 or 25%
4. Peculiar Family Circumstances: Prolonged absence from parents, parental disagreement, great difference in children's ages, family quarrels, previous marriage of one parent.	2 (3%)	5 (3%)	7 or 3%
5. Accidental Causes: Parent misunderstanding child, misunderstanding of adult remarks.	1 (1%)	3 (2%)	4 or 2%

	Male	Female	Total
6. Activity of Mind (Instinct to think): Precocious philosophizing, mere play of imagination, much alone, nothing to do.	11 (16%)	16 (11%)	27 or 13%
7. Growth Stimulus: Early adolescent development, desire for the different.	1 (1%)	2 (1%)	3 or 1%
8. Absence of family resemblance, mental and physical.	3 (4%)	13 (9%)	16 or 7%
9. Miscellaneous: Praise with ulterior motive, self pity, lure of elsewhere or of the wild, mood, circumstances, desire for older brother, vivid dream.	7 (10%)	2 (1%)	9 or 4%
10. Unable to recall.	5 (7%)	13 (9%)	18 or 8%

Of those giving affirmative answers to question No. 3, 214 or 83% answered this question. Of these, 70 were males or 80% of the males answering question No. 3; and 144 were females or 84%. In only two instances is there a noticeable difference here between the sexes. Absence of family resemblances seems to be more significant for the girls and there is a similar preponderance of boys in the group called mental activity or the manifestation of the instinct to think. In passing it is interesting also to observe that all of the ten cases under dissatisfaction had ideas of greatness in their fantasies. This will be discussed further in the next section.

Relation of Fantasy Forms to Causes Given

Observing almost as many reporting notions of inferior parentage as there were of those reporting ideas of greatness, with other indications of developmental stages of the fantasy already mentioned, it seemed possible that a segregation of the causes which each of these groups offered might also be productive. The results of this segregation appear in the following table:—

	Orphan or Foundling Group	Ideas of Greatness
Suggestion	19 or 58%	12 or 30%
Dissatisfaction	0	10 or 25%
Mistreatment	5 or 15%	6 or 15%
Miscellaneous and Indefinite	9 or 27%	12 or 30%

The most conspicuous thing appearing in this comparison is the complete absence of dissatisfaction causes in the "orphan group" and its emphatic presence in the "greatness group." Mistreatment is about equally influential in the two forms of the fantasy, and suggestive causes are given much

more frequently for explanation of the fantasy by those in the "orphan group" than by those in the "greatness group." For help in grasping the meaning of these likenesses and differences reference should be made to the presentation of data concerning the forms of the fantasy. There will be found indications of stages of development, as was also suggested in the answers to the question concerning belief in the fantasy. Following this suggestion of a developing fantasy the table just presented would seem to indicate that mistreatment and suggestion first give rise to the idea of foster-parent-hood which early takes the form of orphanage or inferiority of some sort, perhaps assisted by feelings of helplessness or inferiority in the face of supposed ill usage or partiality for other children. Then with the continuation of the fantasy, growth and the vigor of approaching adolescence come ideas of greatness, doubtless again assisted by the suggestive causes mentioned, and reflection on these ideas of greatness might be the reason for the appearance of the alleged dissatisfaction causes in the "greatness group," the dissatisfaction thus being more consequence than cause. This seems much more likely than that the ideas of greatness should come first and with continuation of the fantasy the dissatisfaction disappear. Comparison with the complete table presenting all causes alleged will show all dissatisfaction cases are found in this table correlated with ideas of greatness, whereas only a small number of the suggestion cause cases and mistreatment cause cases are found here. All dissatisfaction cases in this study had ideas of greatness. Such a statement would be far from the truth for any of the other cause groups. It will be further observed that while the miscellaneous and indefinite group is a considerable percentage of the whole only a most improbable distribution of them, if more definite answers were obtainable, could alter the relationships between the groups.

Age to which Fantasy is Attributed
(Answers to Question No. 7)

The following table presents the ages which 240 of the 258 people who had recallable experience with the fantasy mention as being the approximate time of its occurrence:—

Ages	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Indefinite or No Answer	Total
Males	1	2	5	5	7	5	20	5	10	3	7	3	2	2	10	87
Females	5	8	10	11	28	18	26	7	21	11	6	7	3	2	8	171
Total	6	10	15	16	35	23	46	12	31	14	13	10	5	4	18	258

It will be observed that definite answers were obtained from 93% of the group. Medians for both the male and the female distributions fall in the ten year group. This is undoubtedly too low because of the method used in tabulation. Some gave a definite figure, some said about a certain age and of course in these instances the figure given was used. But there were also many who answered in terms of an approximate range of years, as for example "About ten to twelve." Where this was done the tabulation was by the lowest figure given. Thus the actual measure of central tendency is higher than ten years, probably between ten and twelve years. Of course these are only rough approximations, but it must be remembered that most of these are approximations by young people not very far removed from the period in question, by young people still in their adolescent years (the median for the entire group studied being 18 years).

Effects of the Fantasy upon Conduct
(Answers to Question No. 8)

In this table are presented in summarized manner the answers to that question which sought to discover if any considerable number could recall influences of the fantasy upon their behavior, and in the event of there being such what the nature of the conduct effects might be. Of course the table is based upon the answers of those who had in response to question No. 3 indicated recallable experience with the fantasy.

	Male	Female	Total
No answer given	15 or 17%	27 or 16%	42 or 16%
No conduct effect recalled	55 or 63%	84 or 49%	139 or 54%
Some effect reported (See next table)	17 or 20%	60 or 35%	77 or 30%
Totals	87	171	258

It is worthy of note that this question was answered positively or negatively by 83% of the males and 84% of the females, or 84% of the group reporting experience with the fantasy.

The 77 people or 30% above who reported that the fantasy did have some effect upon their conduct, which they could recall, describe the effect in various ways but with sufficient clarity to make possible an effective grouping. It appears in the table following:—

Nature of the Conduct Effects	Number	Per Cent
Deliberative thought, seeking proof, meditation, etc.	26	34%
Depressing emotional reactions:— Melancholy, embarrassment, fright, etc.	6	8%
Domesticating conduct:— Expression of greater affection for parents (1), conscious imitation of parents (1), improved conduct (1), more considerate conduct (2).	5	6%
Alienating conduct (from parents):— Teasing, tormenting, deceitful, disobedient, revengeful, etc. toward parents (7). Stimulated independence of conduct, running away considered and car ried out, or more self confident (9). Acting part of supposed real self (4) Reserved toward parents (4). Telling of the discovery (3). Seeking discovery by real parents (1). Deliberately acting in strange manner (1). Suspicious, sulky, saucy, snobbish, etc. (8).	37	50%
Miscellaneous (Poetic efforts, and disinclination to study).	2	3%
Positive but indefinite	1	—

The sub-topics in the table just presented are in most instances words and phrases taken directly from the questionnaire returns. In the recollection of these students the predominating effect of the foster-child fantasy upon conduct, or at least its conduct concomitant, is conspicuously that of alienation from the parents. This is of course in clearest harmony with the family romance generalizations of the psycho-analysts. The seeking of proof and meditative thought might be considered as a stage preliminary to the alienation in the fully developed phenomenon. So also the depressing emotional experiences reported. But the tendencies classed above as domesticating indicate a tendency in the other direction. Of course the writer is quite well aware that these may not be effects of the fantasy at all but merely growth concomitants, perhaps causing the fantasy. The writer is here merely seeking and presenting the opinions of his subjects.

As before, the returns in the "orphan group" and the "greatness group" were then isolated and the answers each gave to the question concerning conduct effects was tabulated according to the rubrics just used in order that any significant group peculiarities might again be revealed. The result was as follows:—

Effect Reported	Orphan Group	Greatness Group
Deliberative thought, etc.	6 or 18%	2 or 5%
Depressing emotion	2 or 6%	....
Domesticating conduct	3 or 9%	....
Alienating conduct	7 or 21%	13 or 32%
No conduct effect reported	13 or 40%	17 or 43%
No answer given	2 or 6%	8 or 20%

The great difference between the percentages of the two groups giving no answer to the question makes any generalizations from the other differences hazardous. The other differences suggest, however, a trend away from deliberative thought, depressing emotion and domesticating conduct (early stages perhaps of the fantasy) toward alienating conduct.

Duration of the Fantasy Believed
(Answers to Question No. 9)

People answering the questionnaire who had actually believed themselves to be foster children were asked to indicate as nearly as possible how long the belief continued. The following table summarizes their estimates:—

	Male	Female	Total	Per cent
No answer or too indefinite for use	4	8	12	17%
Very brief time (few minutes to few days)	6	11	17	24%
"Three months," "four months", or "few months".	2	5	7	10%
About one year	2	4	6	8%
About two years	1	13	14	20%
About three years	1	6	7	10%
About four years	..	3	3	..
About five years	..	1	1	..
"Several years"	..	3	3	..
About fifteen years (prolonged separation from parents).	1	..	1	..
Totals	17	54	71	

Some indicated in their answers that the belief was intermittent by the phrase "off and on" for the time specified. The long duration of the believed fantasy is a little surprising. Thirty five or 49% think the belief endured for a year or in some cases much more. It is also of interest that the question was answered in a specific manner by 83% of those reporting belief in the fantasy. Generalizations from these figures would, however, be unreliable because of inconsistencies in reporting. The experience was apparently spasmodic or recurrent with many; and in answer to this question some gave

an estimate of the period within which these recurrences came, while others gave merely the approximate duration of a single occurrence.

Disappearance of the Belief

The tenth question revealed that as a firm belief the fantasy had disappeared in all cases; although two or three report a lingering influence well described in the phrase of the one who said, "At times it seems to haunt me."

The second part of the question asked why the belief had disappeared, of course seeking the opinion of those answering. The answers to this appear in the following table:—

Reasons Given for Disappearance of Believed Fantasy

	Male	Female	Total	Per cent
Growth and development of intelligence, recognition of absurdity of belief, discovery that others had experienced the same thoughts.	9	24	33	46%
Parental intimacy (parental confidence, assurance and explanations and parental affection).	4	17	21	30%
Discovery of proof (Observation of family resemblances, discovery of records, etc.)	..	12	12	17%
Miscellaneous (One vague answer, one where aspects of belief failed to materialize).	..	3	3	..

The numbers in this table refer to the number of times each reason was mentioned. As some were mentioned more than once the numbers will not of course check with the other tables. The percentages are based on the number who believed their fantasy, that is 71. This question was answered by 83% of them in a definite manner. The others did not answer or said they were unable to do so.

Comparison with Results of Psychoanalyses

That inquiry of more than nine hundred young people should reveal twenty-eight per cent who could immediately recall experience with the foster-child fantasy supports the Freudians at the outset. To this number ought of course to be added all those who could immediately recall the experience, those who had it and with whom it is now beyond even mediate recall (instances of which with adequate proof have come to the

writer's attention aside from those here studied), and those in whom it may have suffered a psychoneurotic suppression. Of course such an addition cannot be made in terms of figures; but the mention of such cases indicates the minimal nature of the twenty eight per cent discovered by this study. Thus considered the number is sufficiently large to warrant the assertion that it is a common experience of childhood. So far the psychoanalytic generalization has been justified. And a more definite statement can now be substituted for Rank's that "many persons . . . very frequently remember occasions of this (foster-child) kind."

From psychoanalytic studies the foster-child fantasy was located in the pre-adolescent years (Freud's period of sexual latency or Jung's pre-sexual period). The two hundred and fifty-eight young people found in this study to have had recallable experience with the fantasy located it in the same period of development. The median of the answers given fell between the ages of ten and twelve years. The years eight to twelve inclusive covered 61% of the answers.

Taking Rank's oft quoted statement of the course of the family romance as the authoritative presentation for the psychoanalysts one finds in addition to the basic parental detachment motive of the romance the mention of two immediate causes of the foster-child fantasy, feelings of parental neglect or lack of affection and romantic literature. These two immediate causes have been amply verified by the results of this study. They were the two causes most frequently mentioned by these young people. But there has also been brought to light certain other supplementary causes, not mentioned by Rank, which in the minds of these subjects are of some importance. They are prolonged absence from the parents, marital infelicity observed by the children, precocious philosophizing, lack of companionship and the absence of mental or physical resemblance to the parents. The last is of peculiar interest not only because it is so common a topic of discussion before children but also because the subsequent development of family resemblances is several times mentioned in these returns as a cause for the decline of belief in the fantasy. One of those who had never experienced the fantasy remarked that "the reason I have never acquired such a notion is the fact that all my life every one has spoken of the great resemblance I have for my father."

Rank presents the fantasy as undergoing development from an early stage in which the child thinks of himself as an adopted or step-child to the higher stage characterized by expansive ideas, variously elaborated according to the child's adroitness

and the material at his disposal. This questionnaire study not only supports but also considerably elaborates the psychoanalytic conclusion. All through the returns there are indications of the developmental stages of the fantasy. Concerning the forms of the fantasy there were those, a large group, who had but the vaguest if any idea beyond the thought of foster-childhood, some saying that they had never thought further than that. Then there were those who reported thinking themselves as different characters at different times. The apparent relationship of the different form groups (incipiency, orphan, same social status, great parentage, supernatural being) suggests the same thing. The relationship of forms to causes carefully tabulated indicated the developmental interpretation of the fantasy as the only feasible explanation of the otherwise strange distribution of causes mentioned. The relation of the forms to the reported conduct effects pointed, if less clearly, in the same direction. Examination of the conduct effects indicated stages from meditation on a more or less fascinating idea to decidedly alienating conduct. Degrees of belief also appeared very clearly in the answers given. Even without knowledge of the psychoanalysts conclusions it would have been difficult if not impossible to have interpreted this data otherwise. And the conclusion is in harmony also with the known traits of childhood imaginative fancy as well as the growth of delusions in paranoia.

The forms of the fantasy named herein revealed not only the two mentioned by Rank but also another equally prominent, that of inferior parentage, which seems to lie between the two because it has a definite form and the incipient stage lacks definition. The few cases of fantasies of parentage of similar social status hint at another intermediary stage, decidedly harmonic with the concept of an outward tending libido, which might follow the inferiority stage. And the few cases of those who conceived themselves as of supernatural or very mysterious nature might point to a still higher development of the fantasy, rising to heights above mere concepts of worldly greatness as in paranoid delusions. If true this might be of much interest to those attempting to apply psychoanalytic concepts to the interpretation of the founders of religion. But the writer is far from confident that the last two are genuine stages in the development of the fantasy. The number of cases is small and they might prove to be merely circumstantially conditioned variants from the normal course. For the inferiority stage, however, in which the child thinks of himself as a foundling or poor asylum orphan, following the earlier or incipient stage in which there is no con-

cept beyond the thought of foster-childhood, there is sufficient evidence in the data presented. Of course the demonstration of a stage in the course of the fantasy characterized by inferiority concepts immediately reminds one of Adler, and it might be that ideas of greatness develop as compensations for these inferiority concepts which doubtless are accompanied by feelings of inferiority.

In considering the forms of the fantasy it is interesting to notice Rank's statement concerning the sources of the ideas of greatness. He says: "The child utilizes an accidental coincidence of actual happenings (meetings with the lord of the manor, or the proprietor of the estate, in the country; with the reigning prince, in the city. In the United States with some great statesman, millionaire)" (5, page 65). To the writer there is in that quotation the implication that in this country the fantasy is less likely to take the form of royalty or nobility. Perhaps this is so, but as a matter of fact eleven of the forty cases of expansive ideas, here studied were found to have the form of princes, kings or nobles. The appeal of non-democratic institutions is apparently not dead in the minds of American children.

As has been so often presented in the psychoanalytic literature, the principal motive of the family romance is the progressive detachment from the parents. Rank points out that revenge and retaliation motives also appear in the course of the romance, but considers them as stimuli for further imagings, further developments of the fantasy. The progressive detachment from the parents was clearly indicated in the writer's returns from those who recalled changes in conduct at the time of the fantasy. Many mentioned much thought about the matter and the deliberate seeking of proof for their notions, some wrote of depressing emotions as the consequence of their fantasy, a still smaller number state that they were moved to better behavior and greater affection for their parents (a domesticating tendency), but by far the larger number reported instigation to conduct which would inevitably alienate them from parental influence. The general direction of the conduct effect (or concomitant conduct) was clearly away from parental authority in the recollection of these adolescents. The few who mention a domesticating tendency may indicate a slight hesitation to take the plunge into independence, a slight oscillation at the outset before fixation of growth direction is established. The many reports also of revengeful and retaliatory conduct, the conscious tormenting of the parents, harmonizes perfectly with Rank's statements.

In fine this study seems to have resulted in an amplification as well as a justification of the results of psychoanalytic investigation. Psychoanalytic conclusions concerning the foster-child fantasy have stood the test of checking by a different method of research. At the same time it throws doubt upon the statement of Rank that the psychoanalysis of psychoneurotics is the only tool by which the imaginings of childhood may be studied (5. page 63). It is possible that the questionnaire might by itself never have discovered the foster-child fantasy and the family romance, but this study has demonstrated its usefulness as a tool for the checking of the results of psychoanalytic study and also for their amplification. Rank has compared psychoanalysis with empirical observation, but the questionaire as used here is not empirical observation. It approaches in its request for free personal recollection much nearer to the anamneses of psychoanalysis and has the advantage of being able to bring data readily from numbers large enough to permit of statistical treatment.

Summary and Conclusions

The foster-child fantasy has been demonstrated to be a common experience of childhood. Twenty-eight per cent of the 904 returns here tabulated could immediately recall experience with it.

Twenty five per cent of those who reported experience with the fantasy (7.8% of all) believed in it as a correspondence with facts. The others expressed many varieties of attitude from partial belief to none at all.

Three stages of development of the fantasy were indicated. In 54% the fantasy had never taken any definite form, 15% thought of themselves as poor orphans or foundlings, 18% had developed ideas of greatness. Two other stages were suggested in the returns but by numbers too small for generalization.

Suggestion (45%) and supposed mistreatment (25%) are most frequently given by those questioned as causes for the fantasy. But other causes, although less frequent, were indicated sufficiently often to deserve consideration; precocious thinking (13%), lack of physical or mental family resemblance (7%), and dissatisfaction with home conditions (5%).

The fantasy is located by more than half of those reporting in the period of 8 to 12 years of age.

The most common effect of the fantasy recalled, as described by these subjects, is conduct alienating them from parental influence and authority.

The duration of the fantasy for those who had actually believed themselves foster-children was reported to be more than a year by 49% (maximum 15 years). It was reported as of very brief duration by 24%, and as lasting a few months by 10%.

Growth and the development of intelligence is most often mentioned as the cause of the removal of the fantasy among those who had believed it, but there are two thirds as many mentions of parental intimacy and a smaller number reporting discovery of convincing proof, such as physical or mental similarities and actual records.

Comparison with the psychoanalytic presentation, especially that by Rank, results in both support and amplification of the generalizations from psychoanalysis. The frequency of the fantasy, the age location of it, causes given, two stages of its development, and the progressive detachment from the parents are alike in the results from both methods of study. The questionnaire method has, however, added the discovery of other causes, another stage in its development, some modification of statements concerning sources of the fantasy forms; and has revealed itself to be a serviceable supplement to psychoanalysis.

REFERENCES

1. Abraham, Karl. Dreams and Myths. N. Y., Nervous and Mental Disease Pub., 1913. 74 pp.
2. Adler, Alfred. The Neurotic Constitution. N. Y., Moffat Yard, 1917. 456 pp.
3. Hall, G. Stanley. Jesus, the Christ, in the Light of Psychology. N. Y., Doubleday, 1917. 2 Vols.
4. Jelliffe, S. E. The Technique of Psychoanalysis. N. Y., Nervous and Mental Disease Pub., 1914. 100 pp.
5. Rank, Otto. The Myth of the Birth of the Hero. N. Y., Nervous and Mental Disease Pub., 1914. 100 pp.
6. White, W. A. The Mechanisms of Character Formation. N. Y., Macmillan, 1916. 342 pp.

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A METHOD OF STANDARDIZING THE COLOR VALUE OF THE DAYLIGHT ILLUMINATION OF AN OPTICS ROOM

By C. E. Ferree, G. Rand, and I. A. Haupt, Bryn Mawr College.

In a previous article (Psychol. Rev. 1912, XIX, pp. 364-373) we have given a method of standardizing the intensity of light in an optics room illuminated by daylight and have described provisions for keeping constant the intensity selected as standard. If still more exact conditions are wanted for conducting work on color sensitivity another variable, namely, the changing color of daylight, should perhaps be taken into account. It is the purpose of this paper to describe a method of standardizing the color value of the daylight illumination of a room and of correcting the changing values to the standard value in such optics rooms as we have in our laboratory.

We mean by standardizing here only a means of matching the daylight selected as desirable and of reproducing at any given time as a standard the color value which matches the daylight selected. Our plan does not include primarily a means of determining what is the most desirable color value of daylight to use, although that might well be made a valuable feature of the plan. To be of the greatest service in psychological optics the plan should be both reasonably simple of accomplishment and have a fair degree of precision. In devising a method we have endeavored to keep both of these requirements in mind. Obviously the first step to accomplish is to have a source of light the color value of which can be kept constant within acceptable limits. A well seasoned tungsten light operated by a constant current gives a light of sufficient constancy of composition to serve our purpose.[1] It has

[1] In reply to an inquiry about the constancy of color value of the light emitted by the tungsten filament operated at a constant voltage, also its constancy as compared with the light emitted by the carbon filament, we have the following from the Bureau of Standards. "With regard to the constancy of the color from lamps operated at a constant voltage, it can be said in general that almost any seasoned incandescent lamp will burn a considerable time without any appreciable change in the color of the light, unless the filament is operated at a temperature above the normal. In comparing carbon with tungsten filaments the relative constancy will depend on the conditions. If the two are set at voltages which will give the same color, the tungsten will change much more slowly than the carbon In fact a tungsten filament operated at the normal color of carbon lamps will usually

the advantage too that its changes of color with change of voltage are roughly similar in direction to those which occur in daylight. A second step is to have a means of changing the color value of the light to match the daylight selected as desirable and a comparison surface on which to make the match. The first of these purposes can be accomplished by means of thin gelatine filters of known spectrum transmission, properly selected and combined to give the color values needed; and the second by means of any photometer head which presents a sufficiently good field for the comparison. We have already described a simple and inexpensive photometer for daylight work which with the proper selection of filters can also be made to serve the present purpose very well. For a description of this photometer the reader is referred to this Journal, 1916, XXVII, pp. 335-340. When the proper filters are inserted on the side next to the standard lamp this photometer can be used to make both the photometric and colorimetric comparison with no change either in the apparatus or the adjustment of the lamp. With the instrument set for the color and intensity of light selected as standard it can be determined at a glance whether the illuminaton of the room fulfills the desired requirements with regard to both of these features.

We have stated that the method employed should have a satisfactory degree of precision and sensitivity. In order to get some estimate of the sensitivity of the method or what amounts of color change can be detected at different intensities of illumination, the following experiments were conducted. (1) The light from two well seasoned tungsten lamps of equal watt value (40-watt, type B Mazda) was brought to a color and brightness match at the photometer head. The change in voltage required to produce a just noticeable change in color tone was then determined. The work was conducted in a dark room and for convenience in getting the proper range of intensities the photometer head was removed from its place in the box photometer and mounted on an ordinary photometer bar. In making the determinations one lamp, A, was operated at 107 volts and set at the desired distance from the photometer head. The other lamp, B, was operated at such a voltage and set at such a distance from the head as was needed to give an exact color and brightness match. When

burn thousands of hours with no appreciable change in color. Whether different lamps of the same kind give light of the same color depends on how closely the lamps are kept uniform in manufacture. As furnished on the market at present different tungsten lamps of the same size will be found to be more nearly alike than carbon lamps."

the match was obtained the voltage of lamp A was lowered until a just noticeable difference in color tone could be detected at the screen. A measure was thus had of the amount of color change that could be detected by the instrument in terms of the amount that is produced by a given variation of the voltage of a tungsten lamp the specification and the conditions of operation of which are known. When the voltage of lamp A was lowered the intensity as well as the color value of the light at the photometer head was changed. This required a resetting of the lamp B to bring the two photometric fields to equal brightness. However, as a check on the judgment of just noticeable difference in color tone each observation was made under more than one brightness relation between the two photometric fields. The judgment was not difficult to make. Its precision in fact compares very favorably with that of the photometric judgment. The determination was made with the lamps at different distances from the photometer head,—14, 20, 30, 40 and 50 cm. to give the differences in intensity at which it was desired to make the determinations of colorimetric sensitivity. For the sake of a comparison of the colorimetric sensitivity of the photometer heads more commonly used, the determinations were repeated with Lummer-Brodhun heads of the contrast and disappearance types. The results of these determinations are given in Table I.

TABLE I*

A COMPARISON OF THE SENSITIVITY OF DIFFERENT PHOTOMETER HEADS TO CHANGE IN COLOR OF LIGHT OF TUNGSTEN LAMP (MADZA TYPE B) OPERATED AT 107 VOLTS

Distance of Lamp From Photometer Head (cm.)	Voltage Change of Lamp to Give Just Noticeable Change of Color Tone		
	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head
14	0.25	0.5	1.0
20	0.50	1.0	1.5
30	0.75	1.5	2.5
40	1.00	3.0	3.0
50	1.25	3.5	3.0

*The results of this and the following tables are for Observer R. The main points were verified by a check observer, H.

(2) In the second series of experiments a comparison was made of the sensitivity to change of color tone with the three photometer heads, the Bunsen and the two types of Lummer-Brodhun, when the photometric surfaces were illuminated by the tungsten lamps, natural color, and when they were illuminated by the light from these lamps filtered through gelatines so selected as to match the daylight in one of our optics rooms at 11 A. M. on a clear day. The intensity of light at the photometer head was made the same for both kinds of illumination. The comparison was made at three intensities of illumination, corresponding to those given by the tungsten lamps in the preceding set of experiments when placed at 30, 40 and 50 cm. from the photometer head. The comparison was not made for the other intensities because they could not be obtained on our photometer bar on account of the reduction in intensity produced by passing the light through the daylight filters. That is, the lamps could not be brought nearer to the photometer head than 14 cm. and to match the tungsten lamp at 30 cm. from the head, for example, the lamp in front of which the filter was placed had to be set at a distance of 14.3 cm. The results of this comparison are given in Table II.

TABLE II

A COMPARISON OF THE SENSITIVITY OF DIFFERENT PHOTOMETER HEADS TO CHANGE IN COLOR OF LIGHT OF TUNGSTEN LAMP (MADZA TYPE B) OPERATED AT 107 VOLTS AND OF THIS LIGHT FILTERED TO MATCH DAYLIGHT

Distance of Lamp From Photometer Head Giving Equal Illumination for Each Intensity for		Voltage Change of Lamp to Give Just Noticeable Change of Color Tone					
		Type B Mazda Lamp			Type B Mazda Lamp With Daylight Filter		
Type B Mazda Lamp (cm.)	Type B Mazda Lamp With Daylight Filter (cm.)	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head
30	13.5	0.75	1.5	2.5	0.25	0.50	0.75
40	18.8	1.00	3.0	3.0	0.25	0.50	1.00
50	25.2	1.25	3.5	3.0	0.25	0.75	1.50

(3) A somewhat limited comparison was made of the sensitivity of the Bunsen head to change in color tone when the photometric surfaces were neutral and when they were colored. This was done because the color of some pigments is known to be very sensitive to changes in color value of the

light falling upon them. For the purpose of making this comparison a number of such standard pigments as are commonly found in psychological laboratories were substituted for the white screen of the Bunsen head. Two cases were made of this investigation. (a) Six colored screens were used, selected from the Hering series of papers: the dark red, the orange, the yellow, the yellowish green, the blue-green and the violet; and three from the Milton-Bradley series: the red-violet, the red-violet, tint No. 1 and the orange-red, tint No. 1. The light from the 40-watt lamp was in each case passed through the daylight filter referred to above. The brightness of the photometric surfaces was kept constant in all cases at a value equal to that of the white screen illuminated by the filtered light of the tungsten lamp at a distance of 74 cm. This was the highest brightness that could be obtained with this lamp for the colored screen having the lowest coefficient of reflection. This intensity of illumination was selected in order that the colorimetric comparison should be made in all cases for the same brightness of surfaces compared. The results of this comparison are given in Table III. (b) Since in the preceding

TABLE III

A Comparison of the Colorimetric Sensitivity of the Bunsen Photometer Head When Provided With White and Colored Fields—This Comparison was Made With the Intensities of Light Adjusted to Give in All Cases the Same Brightness of Photometer Fields, Namely, the Brightness of the Blue Pigment (Pigment With Lowest Coefficient of Reflection) Illuminated With the Highest Intensity That Could Be Obtained With the Filtered Light of the Mazda Type B Lamp Operated at 107 Volts

Photometer Field	Distance of lamp (cm.) from photometer head giving brightness of colored fields equal to that of white field illuminated by this lamp with daylight filter at 74 cm.	Voltage change of lamp to give just noticeable change of color tone.
Yellowish-green	43.0	1.0
Violet	15.1	2.0
Red-violet	22.6	3.0
Orange-red, tint No. 1	40.0	3.0
Red-violet, tint No. 1	33.5	4.0
Yellow	50.8	4.0
Orange	38.0	5.0
Dark red	31.5	6.0
Blue	13.8	6.0
Blue-green	27.3	8.0
White	74.0	4.0

experiments the pigments of the higher reflection coefficients had to be used at lower illuminations than the other pigments in order to fulfill the conditions that the colorimetric comparison should be made in all cases on surfaces of equal brightness, it was decided to make a comparison of the most favorable of these colors with the neutral screen at illuminations approximately equal to that used for the color of lowest reflection coefficient in the former experiments. This, it will be remembered, was the highest that could be obtained with the filtered light of the 40-watt lamp. For this purpose the yellowish green, the yellow and orange-red, tint No. 1, screens were used. The results of this comparison are given in Table IV. These results, it scarcely need be pointed out, have a

TABLE IV

A COMPARISON OF THE COLORIMETRIC SENSITIVITY OF THE BUNSEN PHOTOMETER HEAD WHEN PROVIDED WITH WHITE AND THE MORE SENSITIVE COLORED FIELDS USED IN TABLE III, AT INTENSITIES OF ILLUMINATION EQUAL APPROXIMATELY TO THE HIGHEST THAT COULD BE OBTAINED WITH THE FILTERED LIGHT OF THE TYPE B MADZA LAMP OPERATED AT 107 VOLTS

Photometer Field	Distance of lamp (cm.) from photometer head giving equal brightness of white and colored fields for Mazda type B lamp with daylight filter	Voltage change of lamp to give just noticeable change of color tone
Yellowish-green	16.5	0.25
White	26.5	1.50
Yellow	13.5	1.50
White	20.0	1.0
Orange-red, tint No. 1	15	1.50
White	22	1.25
Violet	15.1	2.0
White	74	4.0

much more direct bearing on the problem of selecting screens for our standardizing instrument than those of the former comparison. That is, in the selection of screens for such instrument we are concerned with relative sensitivities at equal illuminations, not equal brightnesses, of screen. Obviously in the final selection of screens for any particular instrument that screen should be chosen which shows the greatest sensitivity for the range of illuminations possible for the instrument or for the range that is likely to be used.

An inspection of the results in Tables I-IV shows the following points.

(1) As might be expected smaller color changes could be detected at the higher than at the lower intensities of illumination. That is, at the higher intensities the color of the light was less saturated (the inhibitive action of the achromatic on the chromatic component of the retinal excitation) therefore a smaller change was needed to be just noticeable.

(2) The colorimetric sensitivity of the photometer heads employed is in the following order from greatest to least,—the Lummer-Brodhun, contrast type; the Lummer-Brodhun, disappearance type; the Bunsen. As bearing on the type of field that gives high colorimetric sensitivity such comparisons are of importance to colorimetry by the monochromatic method.

(3) Smaller changes of voltage were required to produce a noticeable change in the color of the light filtered to match daylight than in the color of the unfiltered light of the Mazda type B lamp.

(4) Of all the colored screens employed the yellowish-green, the violet, the yellow and the orange-red, tint No. 1, showed greater sensitivity to changes of color of light of the type produced in these experiments (changes similar to those which occur in daylight) than the white screen when the comparison was made at equal brightness of screen. When, however the comparison was made at illuminations approximately equal to the highest illumination that could be obtained with the filtered light of the 40-watt lamp only the green showed a greater colorimetric sensitivity than the neutral screen. The green screen seemed to be peculiarly sensitive to the changes which are produced by adding the longer wave-lengths to daylight. That is, the adding of the red wave-lengths appeared to decrease the saturation of the green and the adding of the yellow wave-lengths, to increase the yellow component already present. At the highest intensity of illumination used a change of only 0.25 volt was needed to cause a noticeable change in the color of the screen. Indeed at this intensity of illumination the colorimetric sensitivity of the Bunsen head was so much improved by the substitution of the green screen as to equal that of the more sensitive of the two Lummer-Brodhun heads with the white screen.

As we have already indicated the work of standardizing

for color value and intensity may be done at the same time. It may be accomplished as follows: A daylight is selected of the color value and intensity desired. On the side of the photometer head next to the standard lamp gelatine filters are inserted so chosen that when the light from this lamp run at a given voltage and set at the position on the bar needed to give the intensity match, is filtered through them, the photometric surfaces illuminated by this light match in color value the surface illuminated by the daylight. To reproduce this standard at any future time all that is needed is to reset the lamp in the same position on the bar and to reproduce the voltage. With the photometer set up at the point of work in the room or as near to it as possible, the process of checking up the illumination both as to color value and intensity becomes very simple. A glance at the photometric field is sufficient to give the desired information. Since it is not so easy to arrange for the correction of daylight to the color value chosen as standard as it is to correct the intensity, feasibility may dictate that the work be done within a certain range of variation of color value. The apparatus recommended may be used to standardize this range as follows: If the daylight chosen as desirable be that of skylight near the middle of a clear day the changes that are apt to occur during the course of the day or from day to day may be approximated roughly by lowering the voltage of the standard lamp or by lowering the voltage supplemented by the addition of one or more of the thin gelatine filters properly selected. When this is done and the lamp is reset to compensate for the change of intensity produced, a range of variation of color value is fixed within which the daylight illumination must fall or be rejected for the particular work in hand. In such a case the photometer is set so that the standard surface in the photometer head is illuminated with the light chosen as the limit towards which the color value may vary and still be accepted for the work in hand. It is very easy then at any particular time to judge whether the dominant color of the daylight incident upon the receiving surface of the photometer falls within the limiting value.

Obviously a means of correcting the changing daylight to the color value chosen as desirable would be an advantageous supplement to the work of standardizing. The method we purpose for use for this is as follows: At a distance above the diffusion sash of ground glass installed beneath the sky-

light in our optics rooms, sufficient to give a good spread of light, will be installed opaque pendant reflectors of the distributing type. These reflectors will be supplied with filters which transmit an excess of the short wave-lengths. They will be installed on separate circuits, one or more to the circuit, so that a variable proportion of the artificial light may be used as is needed. Further to vary both the composition and the amount of artificial light used, wall rheostats will be included in as many of the circuits as is found to be advisable. Quite a wide range of composition and intensity of light can be obtained also by the use of lamps of different types and wattages. The artificial units may be installed well out of the road and the desired direction and throw of light be obtained by the shape of the reflector and the angle at which it is installed. Further the amounts of daylight used in getting the desired composition may be controlled by a system of curtains placed under the skylight above the diffusion sash and the artificial units. The elaborate system of curtains which is already installed beneath the diffusion sash in our rooms will serve as they do now for fine gradations of intensity of light and may prove useful to some extent perhaps for the control of the composition.

An alternative to a flexible system of correction of actual daylight for both composition and intensity is the utilization of some one of the artificial daylights which are now on the market. Of the unfiltered sources the carbon dioxide tube gives perhaps by common agreement the closest approximation to skylight. Its cost, however, is prohibitive for the greater number of laboratories. The same thing might well be said of the best of the filter units. The ones most familiar to us all and the most available from the standpoint of cost are the blue bulb lamps. With regard to these lamps, however, only a rough approximation to daylight is claimed. We have thought that it may be of interest to show here a spectrophotometric comparison of one of them, the type C-2 Mazda lamp, and of some of the closer approximations to daylight, with the black body at 5000 degrees absolute which is sometimes taken as the standard of average daylight. (See Fig. I.) For the comparative curves given in this figure we are indebted to the Electrical Testing Laboratories, 80th street and East End avenue, New York City. The comparison here, it will be remembered, is photometric, not colorimetric.

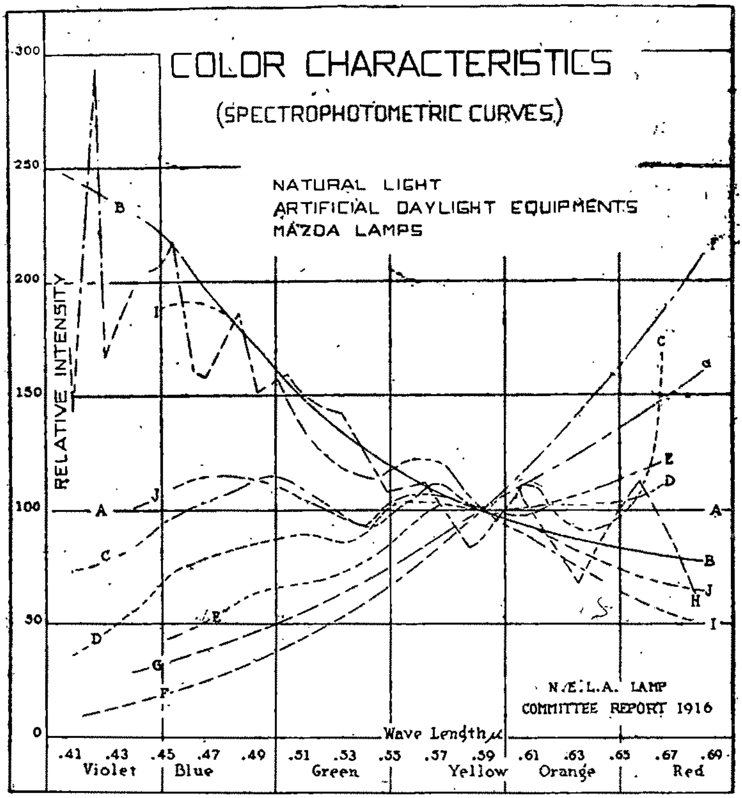

Fig. 1

A—Black body at 5000 degrees absolute ("Average Daylight")
B—Blue sky (Ives) I. E. S. Transactions, 1910, p. 208.
C—Daylight glass with Mazda C lamp (Brady) I. E. S. Transactions, 1914, p. 952.
D—Bluish glass with Mazda C lamp (Sharp) I. E. S. Transactions, 1915, p. 220.
E—Mazda C_2 lamp.
F—Mazda B lamp (7.9 lumens per watt).
G—Mazda C lamp (20 lumens per watt).
H—Moore tube (Paper read before I. E. S. November 11, 1915).
I—Trutint glass, (Luckiesh) I. E. S. Transactions, 1914, p. 839.
J—Trutint glass, (Luckiesh) I. E. S. Transactions, 1914, p. 839.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener, H. P. Weld and K. M. Dallenbach

XLVII. Size *vs.* Intensity as a Determinant of Attention

By Alma M. Bowman

This paper is related to two of the experimental studies on the determination of attention which have been published from the Cornell Laboratory. Meads, working with form *vs.* intensity, obtained from five observers the positive result "that a light-form of relatively low intensity may have as great a power to attract attention as a formless light of relatively high intensity."[1] Curtis and Foster, working with size *vs.* intensity, were unable to reach any positive conclusion. Their three observers gave widely differing results: one showed a positive and one a slightly negative effect of size, while to the third size was apparently indifferent; and in the case in which the effect of size was negative, spatial position proved to be more important than a fourfold increase in area.[2]

It may very well be the case that form is, and size is not, a powerful determinant of attention. If this conclusion is sound, then the diversity of result obtained by Curtis and Foster means simply that in their experiment the effect of size was cut across by other influences which the plan of the experiment did not permit them to bring under control. There is, however, between the two studies a difference in method. Meads employed the method of limits; Curtis and Foster, taking their cue from the report of one of Meads' observers (Professor Weld) that expectation might be a disturbing influence, changed over to the method of constant stimulus-differences.[3] Since now the method of limits has given positive results, and the method of constant stimulus-differences has given results that are positive, negative and indifferent, it seems plainly necessary to attack the problem of size by the method of limits. This is what we have done in the present paper.

Apparatus.

The apparatus was set up, as usual, in a dark room. The observer, screened by heavy black curtains, sat comfortably at a table, his head held firmly in position by an adjustable biting board. Facing him at the distance of approximately 50 cm. was the ground glass plate upon which the two areas and the fixation-point appeared. The stimuli were as before: a standard Greek cross of 56 sq. cm. was compared with similar crosses of half and twice its area. The fixation-point lay between the crosses, at a distance of 10 cm. from each; it was illuminated throughout the experiment, though it was not visible during the exposure of the crosses.

[1] L. G. Meads, "Form *vs.* Intensity as a Determinant of Attention," this Journal, xxvi., 1915, 151.

[2] J. N. Curtis and W. S. Foster, "Size *vs.* Intensity as a Determinant of Attention," this Journal, xxviii., 1917, 294f.

[3] Meads, 151; Curtis and Foster, 293.

Exposure was made by a Wundt gravity tachistoscope set for one-tenth of a second; the actual time, as measured by the Hipp chronoscope, was 100.3 ± 0.7 sigma. Heavy felt pads, laid at the base of the instrument to catch the shutter, made the exposure almost noiseless.[4]

The light was obtained from a 40 watt Mazda Daylight lamp placed in a projection lantern behind the tachistoscope. The rays passed into a diffusion box which was white inside, and whose far end contained two circular ground glass windows, 18 cm. apart and 8 cm. in diameter. The amount of light issuing from these windows was controlled by cardboard shutters, which by a graded series of circular openings carried the illumination by steps of 4% down to 12%, by steps of 2% down to 4%, and by steps of 0.5% down to 0.5%. The illumination of the comparison crosses might be varied within these limits; that of the standard cross remained constant at 20%.[5]

Experiment.

The following instructions, identical with those of the previous work, were read to the observers at the beginning of every experimental hour. "At the signal 'Now' put your attention definitely on the fixation-point. Two crosses of unequal size will be exposed. Judge which of them, if either, is the more clear; that is, which of them catches your attention the more." The method employed, for the reasons given above, was the method of limits. The observers judged 'Right,' 'Left,' or 'Equal.' 'Doubtful' judgments, in accordance with present practice, were thrown out, and the observations were repeated.[6]

The observers were F. L. Dimmick (*D*) and L. B. Hoisington (*H*), assistants in psychology; Myrl Cowdrick (*C*), graduate student in the department; and K. M. Dallenbach (*Da*). *D*, *H*. and *Da* were highly practised in the observation of clearness.[7]

Before the experiment began, a preliminary series was given to every observer in order to determine the critical range. Short warming-up series were also given at the beginning of every experimental hour. The experimental series were arranged for measurement of the space error and for compensation of the irregular influences of practice and fatigue.

Every observer made four series: two with the large cross, standard right and left; and two with the small cross under similar conditions. The series was composed of ten ascending and ten descending observations. The order in which the four series were presented to the observers was as follows:

[4] Meads (150) used a spring tachistoscope with a time of 110 ± 8 sigma; Curtis and Foster (293) used a pendulum tachistoscope with a time of 110 ± 3 sigma.

[5] Meads and Curtis and Foster used an episcotister; but Curtis and Foster found the range of experimental variation insufficient (294). Our arrangement overcomes this difficulty.

[6] S. S. George, "Attitude in Relation to the Psychophysical Judgment," this JOURNAL, xxviii., 1917, 33ff.

[7] *D* is the observer who showed the positive effect of size in the work of Curtis and Foster (293, 295); *H* had observed in a study of the measurement of attention (this JOURNAL, xxvii., 1916, 459); and *Da* had observed in several such studies (this JOURNAL, xxiv., 1913, 465ff.; xxvii., 1916, 443ff.; xxix., 1918, 204ff.).

Series	Observers			
	D	*H*	*Da*	*C*
Large cross, standard right	1	2	3	4
Large cross, standard left	2	3	4	1
Small cross, standard right	3	4	1	2
Small cross, standard left	4	1	2	3

A series was completed within a single experimental hour.

Results.—The results are given in the table below:

O	Position	Small Cross				Large Cross			
		Av.	M. V.	Gen'l Av.	Deviation from stan'd	Av	M. V.	Gen'l Av.	Devia'n from Stand'd
D	right left	53.0 33.4	13.8 8.3	43.2	+23.2	10.9 10.1	4.7 4.7	10.5	—9.5
H	right left	79.2 39.0	4.8 10.6	59.1	+39.1	27.6 2.9	5.8 1.5	15.2	—4.1
Da	right left	40.2 37.6	6.6 5.2	38.9	+18.9	18.0 10.1	2.5 4.3	14.0	—6.0
C	right left	30.6 17.7	5.2 3.1	24.2	+4.2	23.0 15.1	5.6 3.7	19.0	—1.0

This table shows the average per cent. of illumination necessary for the comparison crosses if they are to be equal in clearness to the standard cross. The data of the 5th and 6th, 9th and 10th columns indicate that the small cross, to be as clear as the standard, must be more intense; and that the large cross, to be as clear as the standard, must be less intense. The *direction* of our results is thus constant for all four observers. There is, however, a marked difference between *C* and the other observers: her *plus* and *minus* deviations from the standard are very small. *C*, it may be noted, was a comparatively untrained observer. If we consider only the results of the three observers who were highly trained in the observation of clearness, we find uniformity of tendency, well-marked in all cases, though showing individual variation of the order 2 to 1. For *C*, the *m. v.* is also larger with both crosses than the difference between the standard and comparison stimuli. Even for the trained observers, the *m. v.* with the large cross is not always satisfactory.

But although the direction of the results is constant, the *magnitude of the space error* proves that the effect of size was cut across and obscured by another influence. The data which indicate the effect of position are given in the 3d and 7th columns of the table. For both of the comparison areas and for all observers the left-hand position is the more favored. The position-ratio is 2 to 1 for *H* and *C* with the small cross, and rises as high as 9 to 1 for *H* with the large cross. The left-hand position has, however, no advantage in the matter of relative variation.

Conclusions.—We conclude that the difference between the results obtained by Meads for form and by Curtis and Foster for size is a true difference, not attributable to the difference of the methods employed.[8]

If we may judge by the results of our trained observers, size as determinant of attention may bear to intensity a ratio which varies from 3:1 to 4:3. For a less trained observer size was indifferent.

In the two experiments upon size (that of Curtis and Foster and our own) spatial position proved to be a major disturbing influence. It seems that the left-hand position has an attentional advantage. We are now undertaking a further and more systematic study of this subject.

[8] Cf. also H. Woodrow, "Outline as a Condition of Attention," *Journal Exp. Psychology*, i, 1916, 23 ff.

THE TONOSCOPE AS A MEANS FOR REGISTERING COMBINATION TONES[1]

By Evelyn Gough and Genevieve Robison

The possibility of producing and demonstrating combination tones in the mass of air was clearly established by Helmholtz.[2] He found that the essential condition for the generation of strong objective combination tones was that the same mass of air should be violently agitated by two generating tones simultaneously. The siren and the harmonium, each of which possesses a common wind chest in which the air is set in vibration by all notes played upon it, produced objective combination tones which were extremely powerful.

The presence of the combination tones in the air was demonstrated by the use of vibrating membranes and resonators adjusted to be in unison with these tones. Helmholtz found in using the harmonium that when the two generators were supplied by air from different bellows the objective part which the resonators reinforced was much weaker. When no common air chamber or mechanical connection existed for the two tones, as in the case of tones from two singers, two separate wind instruments, or two violins, the reinforcement of the combination tones by resonators was small and dubious. In such cases the combination tones, which might be clearly audible, he believed to originate, from vibrations in the ear, either in the tympanic membrane or in the ossicles of the middle ear.

Apparatus and Method

The present paper reports experiments showing that the Seashore tonoscope has certain unique advantages as an apparatus for the objective demonstration of combination tones.[3] A description of the apparatus employed may throw light on the results. An early model of the tonoscope driven by a synchronous motor at a speed regulated by a 10 v. d. tuning fork was available. It was energized by a commercial alternating current. For controlling the strength of current closely the rheostat furnished with the tonoscope was supplemented by electric lamps. A manometric capsule, designed with special view to effectiveness with the high pitches of female voices, was substituted for the standard piece. It differs from the standard in certain respects. The capsule is much smaller, the membrane being 12.5 mm. in diameter. The gas supply is admitted at relatively high pressure through a small inlet (.25 mm. in diameter). A relatively large tube (2 mm. in diameter) leads from the gas chamber to the flame tip. The flame tip is one of the small-sized tips which are used on small acetylene hand lights. A gas cock makes possible fine adjustments of the flame.

[1] From the Psychological Laboratory of Smith College. The writers wish to express their indebtedness to Professor D. C. Rogers for his generous assistance in regulating and modifying parts of the apparatus used and for his helpful suggestions in the preparation of this paper.

[2] Helmholtz, Sensations of Tone, pp. 152-159.

[3] Seashore, The Tonoscope, Psychol. Monog., No. 69, 1914, XVI, 1-12.

Ordinary dental dam is used for the membrane. For the purpose of this particular experiment a rubber voice tube 30 cm. long was employed. It was stopped at the end with a cork and sealed. In opposite sides of this tube were inserted two smaller tubes with tin funnels at their free ends for mouth-pieces.

Two tones sung simultaneously into the branches of the voice tube evidently move along a common air passage to the membrane and through it are communicated to a gas chamber and a flickering flame in turn. In passage they strongly agitate a common air chamber, a common gas chamber, the rubber walls of a common voice tube, and a loosely stretched membrane, any one of which would, perhaps, be capable of adding combination tones to two primary tones which are made to vibrate through it.

The purpose of this experiment has been to discover whether or not with this apparatus it is possible to get distinct readings on the tonoscope for vibration rates of tones formed by the combination of two tones sung simultaneously into the voice tube, which are wholly different from the readings produced by either tone sung alone.

The inquiry has so far been confined to difference tones of the first order and summation tones of the first order. Pairs of pitches were selected which could be sung easily and read plainly from the tonoscope, and whose difference or summation tones, if they appeared, would also possess vibration rates within the effective range of this instrument. Variously pitched tuning forks gave the objective pitches which the singers attempted to duplicate at each trial. Each of the two tones was sung and recorded separately, then they were sung together, and a complete reading was taken.

RESULTS

			Pitch from Tuning Fork	Tonoscope Readings
1	Generators	a^1	440 v.d.	146 x 3 = 438 v.d.
				219 x 2 = 438 v.d.
		c^1	264 v.d.	132 x 2 = 264 v.d.
	Difference Tone	f		174 x 1 = 174 v.d.
2	Generators	g^1	396 v.d.	196 x 2 = 392 v.d.
				131 x 3 = 393 v.d.
		b	247½ v.d.	125 x 2 = 250 v.d.
	Difference Tone	d		142 x 1 = 142 v.d.
3	Generators	g^1	396 v.d.	192 x 2 = 384 v.d.
				128 x 3 = 384 v.d.
		a	220 v.d.	110 x 2 = 220 v.d.
	Difference Tone	f		164 x 1 = 164 v.d.
4	Generators	f^1	352 v.d.	175 x 2 = 350 v.d.
				117 x 3 = 351 v.d.
		a	220 v.d.	110 x 2 = 220 v.d.
	Difference Tone	c		131 x 1 = 131 v.d.
5	Generators	b	247½ v.d.	124 x 2 = 248 v.d.
		g	198 v.d.	196 x 1 = 196 v.d.

RESULTS—Continued

		Pitch from Tuning Fork	Tonoscope Readings
Summation Tone	(a^1+)		(Theoretical position at 148 x 3 = 444 v.d.) None observed
6 Generators	a	220 v.d.	114 x 2 = 228 v.d.
	f	176 v.d.	171 x 1 = 171 v.d.
Summation Tone	(g^1)		(Theoretical position at 199 x 2 = 398 v.d. 133 x 3 = 399 v.d.) None observed

With the manometric capsule which was employed in these tests tones whose vibration rates lie within a range of the first, second, and third, multiples of the dot frequencies on the tonoscope are easily read. The patterns corresponding to the different dot frequencies, however, are distinctly different in appearance. Pitches which coincide in rate with a dot frequency within the range of the tonoscope give strong contrasts and bring out the dots in much elongated form and at the same distances apart that they hold on the stationary drum. Those which correspond to multiples of the dot frequencies cause the dots to appear with degree of contrast, separation and elongation which are in inverse proportion to the multiple numbers.

Readings for the difference tones were easily gotten in the tests, and where the generator tones were so chosen as to give difference tones within the range for direct reading, the dots representing these tones stood out with striking clearness. In fact, where the generator tones corresponded to multiples of dot frequencies while the difference tones coincided with them in rate, as in the four illustrations given in the table, the dots representing the difference tones were generally clearer than those representing the generator tones. The correspondence between the tonoscope readings for the generator tones and the vibration numbers engraved on the forks from which the pitches were taken was only approximate. The readings for the difference tones, however, coincide exactly with the calculated differences between the readings for the generators.

The results from the summation tones were entirely negative, no readings corresponding to the summation numbers appearing on the screen, although strong primary tones whose summation effect could have been recorded if sufficiently pronounced were used. Modifications in the apparatus favoring the higher tones or the use of lower primary tones might possibly lead to a different outcome.

In summary: The Seashore tonoscope furnishes a means for the demonstration of difference tones which possesses the following distinct merits. The apparatus furnishes conditions favorable for the generation of strong objective difference tones. Clear visible evidence of the difference tones is present. Objective visible readings are produced for each primary tone separately, and for the difference tone and both generating tones simultaneously. A practically untrained observer can record readings for the three tones accurate to one or two vibrations a second.

References:

1. Helmholtz, H. L. F. Sensations of Tone. Tr. by A. J. Ellis, 1895.
2. Seashore, C. E. The Tonoscope. Psychol. Monog., No. 69, 1914, XVI, 1-12.

BOOK REVIEWS

The Elementary Nervous System. By G. H. PARKER. Philadelphia, J. B. Lippincott Co., 1919. 229 p.

This interesting volume is one of the series of monographs on Experimental Biology which are being prepared by a number of American biologists. Its purpose, according to the author, is to portray by means of experimental and quantitative methods the elementary nervous system as it exists in the simpler animals and in the simpler parts of the more complex forms.

In the introduction a brief comparison is made between the ancient and modern ideas of the nervous system wherein it is shown that the complex mechanism of higher animals, according to modern research, is composed of relatively simple elements, the neurones, arranged upon a comparatively uniform plan which from a physiological standpoint falls into three distinct categories. The first is a *receptor* system which is found in the sense organs that receive the external stimuli and produce the sensory impulses. The second is an *adjustor* system or the central nervous organ that receives the stimuli from the receptors and directs them to the appropriate muscles, glands, etc., for an orderly response. In the more highly organized animals the impressions may be made upon the adjustor system as memories and become a permanent part of the animal's nervous equipment. The third category consists of the *effectors* such as muscles, glands, etc., which enable an animal to react to changes in its environment. These three categories are not found in the nervous system of the lower forms but are shown to have developed from a simple, primitive condition consisting of nothing but an effector system.

The main body of the book is divided into three sections. The first one deals with experiments on the behavior of sponges where no nerve tissue is found. Movements are accomplished by means of cells comparable to a primitive form of smooth muscle fiber or others that represent a stage of differentiation between amoeboid motion and simple muscle contraction. No coordinating system is present anatomically or physiologically. All the evidence collected shows that sponges contain the beginnings of a neuromuscular mechanism with the most primitive constituent, muscle, around which the remainder of the system is supposed to have been evolved. Independent effectors, similar to those found in sponges which are not innervated but are activated by direct stimulation, are shown to occur in the higher animals. The examples cited are the iris of the eye, heart muscle, amnion of embryo chicks and acontia of sea anemones. Also sluggish forms of transmission exhibited by the ordinary tissues of the sponge are found in the more complex organisms as in their ciliated epithelium.

The second section of the book is concerned with receptor-effector systems that mark the second step in the evolution of the nervous system. In the sea anemone the neuromuscular structure is composed of a dense meshwork of neurofibrils extending from the outer body layer to the more important systems of muscles in the mesenteries. In these animals the effector systems are more numerous and complicated than in the sponges. They consist of mucous glands, ciliated

epithelia, nematocysts and muscles. Of these, however, the first three are without nervous influence and are, therefore, independent effectors. In the jellyfishes a marked increase in sensitivity is found over that manifested in sponges due to the acquirement of sense organs or receptors and the establishment of nerve connections that create a quicker and more efficient system by increasing the sensitiveness of the new member, the receptor, rather than on a considerable change in the original member, the effector. From the anatomical standpoint this primitive nervous system as seen in the Coelenterates is diffuse, in the form of a nerve net, but it shows some polarity by forming bands or trunks with some resemblance to nerves that permit receptive cells to control muscles or effectors situated some distance from them. Physiologically, the parts are relatively independent with no central control.

In the third section of the book the author discusses the anatomical and physiological relation of the elementary nervous system of the sponges and Coelenterates to the central nervous system of the more complex animals where a new element arises in the form of a central organ or adjustor in the region between the receptor and effector. It is this portion that, in the higher animals and man, has given rise to the central nervous system with all its complexity. In conclusion he says, "Thus the system that arose secondarily around an independent effector, muscle, has in the end gained such supremacy as to take to itself a number of independent effectors, any one of which might in the beginning have served as the nucleus around which the first nervous tissue could have taken origin."

The argument is based upon minute anatomical evidence and a great mass of data collected from numerous ingenious experiments, most of which are very simple in nature but at the same time furnish evidence that leads to definite conclusions. The subject is presented with unusual clearness and accuracy of detail and in such a simple and interesting manner that persons with no more than an elementary training in biology can read this monograph with profit. It is a contribution of utmost importance to the origin and evolution of the nervous system.

I. A. FIELD.

El Psicoanalisis. By HONORARIO F. DELGADO. Lima, 1919. 58 p. Also *La Psicologia de la Locura.* Madrid, 1919. 20 p.

Dr. Delgado's monograph on psychoanalysis is very inclusive and shows a thorough theoretic knowledge of the subject. His reading of both German and English literature has obviously been very extensive, and has not been limited to any one school, so that in his own synthetic presentation, although he follows Freud very closely, he does not neglect the contributions of such men as Adler, Jung, Maeder et als.

The first chapter of Dr. Delgado's *El Psicoanalisis* is devoted to an outline of the Freudian views of the rôle of the sex instinct in human life. Broadly speaking, it deals with the subjects of infantile sexuality, narcissism, Oedipus and Electra complexes, arrest of the libido, repression, the dream mechanism, symbolisation, etc. The application of these principles in the explanation of such perversions as homosexuality is adequately set forth, and the necessity of a natural development of the libido for the formation of a normal personality is emphasized.

The second chapter of the monograph is a brief but clear presentation of the theory of the sexual etiology of the psychoneuroses and allied mental diseases. The anxiety neurosis, neurasthenia, hysteria, compulsive neuroses, paranoia, dementia praecox, and manic-depres-

sive psychoses receive separate consideration in a manner which shows that the author has grasped the full implications of the psychoanalytic doctrines. A little peculiar, however, may appear the inclusion in the same category of the *defence mechanism,* which is usually considered with the other Freudian mechanisms described in the preceding chapter.

The third chapter gives an account of the methodology and technique of the psychoanalytic therapy. The last two chapters are of a more general nature, and are devoted to a defence of the psychoanalytic theory and practice, and to a rather philosophical discussion of the new dynamic conception of the psychic life which the psychoanalytic statements involve.

The small pamphlet called *La Psicologia de la Locura* (The Psychology of Madness), is a brief statement of the new psychological data concerning this problem which the psychoanalysts have given us. It is written in popular style, and does not pretend to have the scope of the larger monograph. The latter is to be highly recommended to students best acquainted with the Spanish language both as a summary of the subject and as a guide to other literature in the same field.

PHYLLIS BLANCHARD.

BOOK NOTES

The Psychology of Musical Talent. By CARL EMIL SEASHORE. Boston, Silver, Burdett and Co., 1919. 288 p.

This comprehensive monograph is addressed to students of applied psychology. Its content must appeal to all interested in music, and teachers and students of childhood should also find much material of value in it. For years the author has devoted himself to this subject, and he is our chief American authority upon it, so that psychology may well congratulate itself that it has here the carefully drawn-up statement of the results of long and constant study.

After treating of the psychology of the musical mind the author then discusses in subsequent chapters the sense of pitch, of intensity, time, rhythm, timbre, consonance, auditory space, voluntary motor control, musical action, musical imagery and imagination; musical memory, intellect, and feeling; and the individual and training in the art.

Human Psychology. By HOWARD C. WARREN. Boston, Houghton Mifflin Co., 1919. 460 p.

This book has no preface or introduction. It has a long list of acknowledgments. It is evidently the material of the author's course of instruction well digested by many years of active teaching. It is abundantly illustrated and has a great many tables. The following are the chief chapters treated: The Science of Psychology; The Organism; The Neuro-Terminal Mechanism; Physiology of the Neuron; Stimulation, Adjustment, and Response; Behavior; Conscious Experience; The Senses; The Components of Mental States; Primary and Secondary Mental States; Succession of Mental States; Attitudes; Character and Personality; Organized Mental Life; with an Appendix on The Mind-Body Relation, Mechanism and Purpose, Neural Activity, and The Visual Process.

The Psychology of Nationality and Internationalism. By W. B. PILLSBURY. N. Y., D. Appleton and Co., 1919. 314 p.

The writer's work is more or less suggested by Graham Wallas, and his position is a compromise between that of MacDougall, with his insistence on immediate instinct, and that of Trotter, who explains all social phenomena by the fear of the individual for the social whole, with the consequent dominance of convention. The chief chapters are: The Problem of Nationality; The Nation as a Psychological Unit—Social Instincts; Hate as a Social Force; Nationality in History; Nationality in the Process of Naturalization; The Nation and the Mob Consciousness; The National Mind; The Nation as Ideal; Nationality and the State; and Nationality and Super-Nationality as Expressed in a League of Nations.

An Outline of Abnormal Psychology. By JAMES WINFRED BRIDGES. Columbus, R. G. Adams and Co., 1919. 126 p.

Nothing could be more admirable, timely, or needed than an outline of this kind. Perhaps no two people would have made the same

tabulation and classification of subject matter, and some would take exception even to his division of it into three parts: (1) The Abnormal Phenomena; (2) The Mental Syndromes or Symptom-Complexes of Insanity; and (3) The Borderline Diseases: Psychoneuroses and Epilepsies. It would be also easy to quarrel with his bibliography, which indicates that he is warped a little by his partisanship for introspection on the one hand and that he is not acquainted with much of the best recent German literature on some of the topics. This perhaps is the chief defect of such an outline. But after all this is said, while the author's classification of abnormal phenomena is not that to which most adhere now, everyone ought to be thankful for this handbook, which is so timely and so conscientiously wrought out.

Sinnesphysiologische Untersuchungen. By JULIUS PIKLER. Leipzig, Johann Ambrosius Barth, 1917. 515 p.

Here we have an admirable and comprehensive survey of the results of the experiments in sense physiology up to the present time with an attempt to synthetize and balance accounts as to what is known and unknown, valuable and otherwise. It is a work of great value, and we hope to have it more fully reviewed at some future time in these pages.

The Voyage of a Vice-Chancellor. By ARTHUR EVERETT SHIPLEY. N. Y., G. P. Putnam's Sons, 1919.

These are extracts from a private diary the author wrote while on an extensive tour of the United States in 1918 as a member of the British University Mission. It is difficult to see any *raison d'être* for such a book. Most of it consists of jottings calendar-wise, giving crude impressions of his trip to this country, while a chapter ambitiously headed "University Education in the United States" gives the crudest and most superficial of all views. To not a few American educators upon whom the Vice-Chancellor made any impression at all he seemed to carry an English atmosphere so dense and impervious as to make him almost inaccessible to new impressions, a man who loved to talk and did so with an English accent delightful to many, but tedious and prolix. It is significant and ominous to our minds that this much-heralded British commission should have devoted so much time to matters so entirely irrelevant to the purpose of the commission, although, so far as we can see, this trivial book may be one of the chief results of the pedagogic expedition.

The Field of Philosophy. By JOSEPH A. LEIGHTON. Columbus, R. G. Adams and Co., 1919. 485 p. (Second revised and enlarged edition.)

This work, the publisher tells us, "gives a concise, clear and comprehensive account of the principal views of the nature of the world and the human soul, and of the meaning of thought and human life, advanced by thinkers in the chief ages of European and American thought down to the present time." Although comprehensive itself, it "furnishes ample guidance for further study."

The first part, which is historical, is rather full, down to and including Scholasticism, with which it stops. Part II consists of the problems of modern philosophy, with a chapter to each of the following: Reality, Dualism, Materialism, Kant, Spiritualism or Idealism, The One and the Many, Evolution and Teleology, The Self, Concepts of Metaphysics, Epistemology, Criteria of Truth, Values, Philosophy

of History, Philosophic Progress and Discipline, and Current Issues in Regard to Consciousness, Intelligence, and Reality.

The Health of the Teacher. By WILLIAM ESTABROOK CHANCELLOR. Chicago, Forbes and Co., 1919. 307 p.

The purpose of this book is to guide teachers in the care of their own health while teaching, which is a serious problem. It means increased strength and efficiency for the day's work, overcoming tendencies to disease, increasing the joy of life, and postponing death. The thirty-eight chapters are divided into parts: (1) Principles of Diagnosis and Cases, and (2) The Rationale of Health Control. Part I includes chapters on The Instinct to be Well, Differential Diagnosis, and on eighteen different troubles with case illustrations of each. Part II is on sleep, diet, exercise, etc., and on the care of the different parts, organs, and functions.

Brightness and Dullness in Children. By HERBERT WOODROW. Philadelphia, J. B. Lippincott Co., 1919. 313 p.

The chief chapters are: The Measurement of Intelligence; Brightness and Dullness; Brains; Physical Defects; Anatomical Age; Pedagogical Age; Simple Mental Processes; Association, Memory, and Attention; Complex Mental Processes; Mental Organization; Heredity; The Organization of Education; and Educational Methods. This book is in the Lippincott Educational Guides series edited by W. F. Russell, Ph.D.

Etude-Critique sur L'Evolution des Idées Relatives à la Nature des Hallucinations Vraies. By RAOUL MOURGUE. Paris, Jouve et Cie., 1919. 66 p.

This somewhat voluminous author writes here rather systematically his conclusions, based largely upon the French writers, that there is great difference in the degree of the hallucinated image and that this is favored by every anatomical reference. He is evidently a rather extreme disciple of Moreau in believing that crepuscular states play a great rôle here. The discussion, on the whole, does not bring us into very close relation with the subject; nor does it suggest much that is new.

Aces for Industry. By RUFUS STEELE. Boston, Houghton-Mifflin Co. 93 p.

This is an indexless, chapterless, and so far as we can see rather aimless but sprightly, gossipy booklet with considerable conversation and plenty of idealism, a book about which it is impossible to form any opinion without reading it all; and the reviewer, after many attempts, can find no incentive to do this. The publisher calls it "an absorbing story of a big new idea in American life," viz., that the chief need of industry is workers with the spirit of the aviator, not content to do a day's work in routine fashion but eager to improve everything.

Aesthetics: A Critical Theory of Art. By HENRY G. HARTMAN. Columbus, R. G. Adams and Co., 1919. 250 p.

The writer here attempts to answer the question, what are the elements of individual interest in painting, music, and poetry? He does not aim so much to increase our appreciation as to correct the old methods of determining substance, origin, and value of art. He does

not deem it unitary, and thinks the psychological or subjective factors have been forced into one-sided prominence, so that our art psychology needs reconstruction. The chapters are the following: Beauty and Art; Art and Theory; What is Art? The Substance of Art; Formulas and Methods in Art-Theory; Painting; Poetry; and Music.

Business Inspirations. By CHRISTIAN D. LARSON. N. Y., Thomas Y. Crowell Co., 1919. 163 p.

Of the twenty-seven chapters of this book the typical ones are headed: Follow the High Vision, The Magic of Sincerity, It Pays to Look Well, What Makes Men Great, Giving Your Whole Self, How to Work, Creating the Right Idea, The Will to Do More. It is written in a sort of hortatory way and is related to other books on business somewhat as the work of a good revivalist is related to that of a stated pastor.

Proceedings of the American Medico-Psychological Society. Vol. XXV, June, 1918. Baltimore, American Medico-Psychological Assn., 1918. 397 p.

Prehistoric Villages, Castles, and Towers of Southwestern Colorado. By J. WALTER FEWKES. (Bureau of American Ethnology, Bulletin 70.) Wash., Govt. Printing Office, 1919. 79 p. (33 plates.)

Handbook of Aboriginal American Antiquities. By WILLIAM H. HOLMES. (Bureau of American Ethnology, Bulletin 60, Part I.) Wash., Govt. Printing Office, 1919. 380 p.

PRIZE IN PSYCHOPHYSICS

The prize of $100 offered in 1914 for the best paper on the Availability of Pearson's Formulae for Psychophysics (this JOURNAL, XXV., 148), has been awarded to Dr. Godfrey H. Thomson, Armstrong College, Newcastle-upon-Tyne, for a paper entitled "On the Application of Pearson's Methods of Curve-Fitting to the Problems of Psychophysics, especially to the Data of Urban's Experiments on Lifted Weights: in four parts, together with Part V. On the Use of Compound Curves in the Analysis of Heterogeneous Material, and Part VI. On an Outline of an Attempt to Make a Generalised Psychometric Function."

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SIR THOMAS WRIGHTSON'S THEORY OF HEARING[1]

By E. G. BORING AND E. B. TITCHENER

The Helmholtz theory of audition, in spite of Ebbinghaus' gallant effort to save it, seems to have sunk beyond recovery. The historian of science must always regard it with admiration, since for combined range and detail and directness of correlation it is still without a rival; and indeed, as model of what a theory 'ought' to be, it will probably figure for many years to come—with the more or less emphatic warning that it need not be believed—in textbooks of physiology and psychology. Helmholtz himself, eminent in physiology and yet more eminent in mathematics and physics, was (we used to think) a living guarantee of the rightness of his theory, a physical theory of a mechanical organ: we might mistrust his theory of vision, but we could trust the physiology and physics of the *Tonempfindungen*. Little by little, however, acceptance has given way to doubt; the modern physicist will not be pinned down to Helmholtz' 'nothing but' sympathetic vibration, and objects positively that sympathetic vibration is put out of court by the dimensions and structure of the internal ear; the

[1] An Enquiry into the Analytical Mechanism of the Internal Ear, by Sir Thomas Wrightson, Bart., Memb. Inst. C. E., with an Appendix on the Anatomy of the Parts Concerned by Arthur Keith, M. D., F. R. S. London, Macmillan & Co., Ltd., 1918. Pp. xi.+155+Appendix (156-254)+9 Plates. See also the account in *Proc. Roy. Soc. Med.*, 12, 1919, Otology Sect., 80-94; and the discussion by Bayliss, Keith, Wrightson, Perrett and Rayleigh, *Nature,* 102, 1918, 124f., 164f., 184f., 225, 263f., 304, 325.

modern physiologist looks in vain for Helmholtz' resonators; and the modern psychologist, oppressed by the number and complexity of his facts of observation, wishes to clean his slate of theory and to start afresh from a thoroughgoing study of the sense-organ, biological, anatomical and physiological.

Meantime, of course, theories flourish, and must be considered as they come. The theory set forth in the book before us is that of an engineer, who approaches the ear "as a piece of working machinery." First conceived as long ago as 1876, it has now been wrought out in detail; and the author has sought to clinch its application by calling in the aid of an anatomist. The combination of forces is unusual; the disregard of psychology is—to a psychologist—refreshing; and we begin to read with a hopeful curiosity. May not this frankly teleological attitude toward the sense-organ, as a machine in being, put us all, physiologists and psychologists alike, on the right track?

Whether or not this hope is eventually to be fulfilled, we soon discover that the writer has not troubled to make things easy for us. His style appears at first to be clear and simple, but in fact it is neither the one nor the other. His physical terminology, in particular, is at best misleading and at worst incorrect. The terms pressure, force, power, work, and momentum are used loosely despite their precise physical meanings;[2] and discussion that begins thus inexactly ends, as it must, in positive error.[3] Moreover, the exposition of the theory is annoyingly unsystematic. We find digressions, repetitions that are sometimes almost verbal, and a general absence of the coordination that might have been effected by literary organization or by cross-referencing. Again and again the reader fails to divine the author's purpose; when is he seeking an account of the analytical mechanics of the internal ear, and when the establishment of a theory of hearing? Much of the description belongs only to the former context, and much of the discussion only to the latter, and the resulting confusion has to be cleared up by re-reading.

The burden of comprehension is thus heavier than it need

[2] *Cf., e. g.*, the confusion of work and power, pp. 134 f. The reader will find plentiful instances of a like kind.

[3] Consider *e. g.*, the application of the energetic principle of conservation to forces which should be dealt with as equilibrated, and the consequent discovery of a supposititious residual force effective in the cochlea: p. 94, lines 9 ff.

be. We do not desire, however, to make too much of this criticism. The book is a war-book; the preface is dated February, 1917; and the writer may have had scant leisure for polishing his material. The looseness of terminology is a more serious matter. We proceed to show, in a number of instances, that it means a fatal looseness of thought.

1. In his preliminary analysis of compound wave-forms [4] Wrightson shows that, if the simple wave-forms be represented by straight lines forming a series of V's, the algebraic addition of two simple wave-forms respectively of frequency x and frequency y will give a compound wave-form, which has $(x+y)$ and also $(x-y)$ approximately equidistant crossing-points (intersections of wave-form with horizontal axis). These two periodic series show no significant interrelation. Wrightson attributes to the $(x+y)$ series the summational tone, and to the $(x-y)$ series the differential tone. The correlation is, however, unsatisfactory. It is to be presumed—and Wrightson seems to imply—that similar properties in the wave-form underlie both simple tones and combinational tones. But the tone x is heard for a wave-form that has $2x$ crossing-points (a complete wave crosses the axis twice); and a tone y is heard for a wave with $2y$ crossing-points. If $(x+y)$ be the summational tone, then it might be expected to have $2(x+y)$ crossing-points. The differential, in like manner, should be conditioned upon $2(x-y)$ crossing-points. Wrightson's $(x+y)$ and $(x-y)$ are each of them an octave too low. The $(x+y)$ crossing-points, moreover, represent an average frequency, and not a sum.

2. This discussion of triangular wave-forms is preliminary. We come presently to the compounding of tones of sine-form. Here we learn that, if we regard every crest, hollow, and crossing-point as an " impulse point," we have possessed ourselves of a key to an understanding of the ear's analysis of compound waves. More concretely: if we build up a compound wave-form by the algebraic summation of sinusoidal components, and note in the complex form the positions of all crests, hollows, and crossing-points, we discover that a great many of these critical points are separated by distances which are approximately the same as the wave-length of one component; that many other pairs of points are separated approximately by the wave-length of the other component; and that

[4] Pp. 12-17; Plate I.

still other points show separations which correspond with the summational tone or the differential tone. This discovery justifies, we are told, the initial assumption that crossing-points, crests, and hollows all function in the same way.

A little reflection, however, raises the question why Wrightson, in these graphical experiments, should wish to find the *wave-lengths* of the components reproduced in the compound. In a simple tone there are two crossing-points, one crest, and one hollow, in every single wave; four impulse points in all. A tone of x vibrations per sec. has $4x$ impulses within the second. It would seem, therefore, that Wrightson should have made his analysis of the compound wave by seeking out recurrences of the quarter wave-length rather than of the wave-length. Whether an analysis with respect to quarter-waves would fail we can not tell. The original curves in the publication of 1907,[5] which unfortunately is not at present available to us, must be analyzed again (as they were originally) by a cut-and-try method.

3. There are more difficulties of the same order, but they appear at higher levels of the argument. We must observe next a fundamental assumption of a different kind.

Wrightson, in his analysis of the hydraulics of the inner ear, ordinarily uses hydrostatic and not hydrodynamic principles. He employs the terms power, work, and momentum; but the dynamic principles that these terms imply do not dominate the discussion. There is no reference to the inertia of the moving parts, although the difference in phase of the curve of air-pressure and the curve of air-displacement[6] is a matter of inertia. Wrightson draws the analogy to the steam-engine as if the work transmitted to the hair-cells were the vehicle of sensation.[7] The engineer's interest in the steam-engine, however, lies ultimately, not in its work, but in its power. Power is rate of doing work; and both the frequency and the amplitude of a tone must be considered if mechanical power is the key to hearing. An engineer determines mean effective pressure by putting a steam-indicator on the steam-cylinder, just as Wrightson wishes might be done with the inner ear;[8] but the engineer does not then forget to measure the speed of the shaft.

The substitution of static for dynamic principles begins in Wrightson's argument when we pass from the sounding air to

[5] P. 31.
[6] P. 76.
[7] Pp. 94, 134.
[8] Pp. 1, 134 f.

the mechanical system of the middle and inner ears[9]. Wrightson shows the waves of pressure and displacement separated in the air,—a dynamical separation. He then notes that the stapes moves as the result of air-pressure upon it, and assumes that it will therefore move *with* the air-pressure.[10] In a series of hydrostatic equilibria it would so move; the position of the stapes would be a function of the pressure upon it. But hydrodynamically there would be a lag due to inertia. An imponderable stapes in air would move with the ponderable air and not with the pressure of the air. An actual stapes weighted by a mass of liquid would similarly be out of phase with air-pressure.

In another passage we read: "As liquid pressure in a closed vessel exerts itself equally in every direction the unit pressure at any one moment which enters the cochlea is not affected by the shape of the vessel."[11] This misstatement is symptomatic of the entire hydraulic account of the inner ear. The law of equal pressures is the hydrostatic law. Engineers make use in the Venturi meter of the fact that pressures are not everywhere equal within a moving liquid, and that the shape of the containing vessel determines the rate of movement and thus the pressure.

The fundamental principle of hydrostatics is the equilibration of forces. Every body is considered in equilibrium and the sum of the forces acting upon it equated to zero. This mode of working is not adequate in hydrodynamics; but it is the mode that Wrightson uses;[12] and it makes difficulty later on.

If it is argued that the masses involved are so minute that their inertia is neglible, we reply that the masses are not disappearingly small and that their rates of motion are very great. Moreover, we must remember that Wrightson's theory stands or falls with the assumption that the ordinary principles of mechanics apply.

4. If we grant the dynamical nature of the problem, we must ask why Wrightson supposes that the movement within the inner ear is a mass-displacement of the lymph and not a transmission of sound-waves, since water does in fact conduct sound. Bayliss has already raised this question.[12a] Wrightson's answer is misleading. He holds that the lymph is "(at

[9] P. 76.
[10] Fig. 30.
[11] P. 47.
[12] *Cf.* pp. 80-87, esp. 83 ff., 86; 124.
[12a] W. M. Bayliss, *Nature,* 102, 1918, 124, 263.

the minute pressures dealt with) inelastic and incompressible, and will therefore be moved instantaneously in the passages of the cochlea."[13] He quotes Helmholtz as authority.[14] He is especially insistent that a liquid is inelastic.[15]

As a matter of fact, however, liquids are not absolutely incompressible and they are highly elastic. Elasticity and compressibility are *inversely* related;[16] a liquid is highly elastic because it is relatively incompressible. The true answer to Bayliss' question has been given by Rayleigh; the cochlea is so short with respect to the wave-lengths of ordinary audible tones that there could be very little difference of phase between the pressures at the oval window and at the round window, and transmission would therefore be practically instantaneous.[17]

5. We may include as a separate item in this list the fact noted above, that the curve of displacement of the stapes can not be, as Wrightson believes,[18] synchronous with the curve of air-pressure. His assumption that the two are synchronous sets the stage for the difficulties of all the discussion that follows.[19] Here the neglect of the dynamic principle is especially unfortunate because of the critical part played by the conclusion.

6. Thus we come to the most important part of the theory, the demonstration that every complete wave of pressure in the air, consisting of a positive and a negative phase, is correlated with the two positive phases and two negative phases of a pressure-curve within the cochlea. The reader must refer to Wrightson's book for the details of the argument.[20]

If we take Wrightson at his own level of exposition, we are at once entitled to object to the development of Fig. 34. Here (by analogy to Fig. 33) " C" must be the mid-position and " B " the extreme position of the stapes. The generating time-circle is struck with " B " as a center; so that the stapes, in moving from " C " to " B," is shown as starting from rest at " C " and moving faster and faster until it reaches " B," where it changes direction and moves back on the return

[13] Pp. vii, 80, 90, 115.

[14] Notes on pp. 56, 74; p. 130; and *Nature*, 102, 184.

[15] Pp. vii, 77, 121, 128, 130.

[16] Elasticity=stress/strain=compressing force/amount of compression. The less the compression produced by a given force the greater the elasticity. *Cf.* A. Winkelmann, *Handbuch der Physik*, I, 1906, 497-503.

[17] *Nature*, 102, 304.

[18] P. 76.

[19] Pp. 76-96.

[20] Pp. 80-84, Fig. 34 and Plate V.

stroke. Such a movement of the stapes is, however, manifestly impossible. The stapes can not reverse the direction of its movement at maximal speed; it must, like any other object in pendular motion, slow down and pass through rest at the moment of change. Moreover, Wrightson elsewhere notes that it halts in the extreme position. [21] Yet Fig. 34 shows it moving over a maximal space in the last increment of time!

The remedy is simple. The time-circle of Fig. 34 must be struck with its center at " C." (Where else indeed should the center of the generating circle for simple harmonic motion be placed than at the center, " C ", of the total displacement?) The stapes, like a swing, moves fastest at its mid-position " C," slows down, and reverses direction at " B." It does not, as Wrightson maintains, stop momentarily at " C " as well as at " B." What, we wonder, has led Wrightson to believe in this arrest of motion at " C "? The examples of reciprocating motion with which the engineer is familiar furnish no ground for the belief. It can not arise in consequence of the invocation of Hooke's law; for it is formulated prior to the mention of Hooke's law, in order that the combined effect of Hooke's law and the twofold arrest may be worked out. Probably the error springs from the assumption that stapedial displacement is an immediate function of pressure (the fifth point mentioned above); for the pressure is zero in the mid-position, and Wrightson seems to think that the resultant velocity must also be zero. He forgets, it appears, that a swing moves fastest in its mid-position, when the effective component of gravity is zero.

If we dissent from Wrightson's reasoning, and consider the stapes as moving fastest in the mid-position, we must strike the generating circle about " C." The curve of elastic resistance, as thus developed, becomes a sine curve and not a versine. If we then take the difference between the imposed pressure (sine curve) and the opposing elastic pressure (now shown to be also a sine curve)[22] we get as the curve of transmitted pressure, not Wrightson's two positive phases followed by two negative phases,[23] but zero pressure. This is exactly the result which could have been foreseen. We are dealing implicitly with hydrostatics and forces in equilibrium. Under Hooke's law and these assumptions the elastic membranes will stretch until they give back a reacting force equal to the impressing

[21] *Cf.* pp. 87, 121, 142.
[22] *Cf.* Plate V.
[23] The red curve of Plate V.

force. Thus we demonstrate Newton's third law. Action and reaction are discovered equal!

It stands to reason that no pressure could be transmitted through the *fenestra ovalis* if there were nothing beyond to press against,—granted always that the pressures due to inertia and change of velocity are, with Wrightson, left out of account. But in point of fact there are elastic pressures arising within the inner ear. Wrightson admits them,[24] but regards them as neglible in comparison with the tensions of the tensor tympani and the stapedius. They are the elastic pressures from Reissner's membrane, the tectorial membrane, the basilar membrane, and the *fenestra rotunda,* all of which are displaced with the stapes. Far from being neglible, they are the determinants of internal pressure within the cochlea, which is of their order of magnitude. The pressure in any canal is the pressure necessary to displace the membranes lying beyond it. In the cochlear duct, the hydrostatic pressure would be atmospheric pressure *plus* the pressures due to the distension of the basilar membrane and round window. In the tympanic canal the pressure would be atmospheric pressure *plus* the pressure from the round window only. In a dynamic system the pressures would depend in addition upon the rate of motion and the inertia of the cochlear lymph. In any case, however, they would be approximately sinusoidal, and they must be accounted for by conditions within the cochlea, and not as a residual left over from the operation of the mechanism of the middle ear.[25]

7. The central feature of the Wrightson theory is this complete cessation of pressure and motion at four points within the complete period.[26] These critical points are points of zero pressure in the doubled cochlear wave, and they are correlated with the crests, hollows, and crossing-points of the air-wave. Thus we get an inkling of the significance of impulse points in Wrightson's analysis of mechanically compounded curves in the earlier part of his book.[27] We saw, however, that his analysis is faulty because the four-fold total wave, instead of the significant quarter wave-length, is laid off among the

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[24] Pp. 144, 154.

[25] This whole discussion of a residual pressure which is delivered to the elastic membranes and not taken up by them, so that something remains over for further use, sounds very much as if work or energy were in question. For there is no principle of the conservation of pressure! But the main argument applies to pressure; and if Wrightson shifts his meanings without changing his words, the reader can not be held responsible.

[26] Pp. 87, 121, 124, 142, 196, 212, 218.

[27] Pp. 26-35, 60-71.

impulse points of the compound curve. We see here that the mechanical significance of the impulse points fails of establishment: We still can not deny the graphical coincidences of the curves, but we may deny that they have brought us far toward an understanding of hearing.

8. If the cochlear curve of pressure is not doubled, there is no "subjective octave."[28] "Subjectively," *i. e.* in the cochlea, there are just as many crests and troughs and crossing-points as there are "objectively" at the tympanic membrane. But even if the cochlear curve is doubled, as Wrightson would have it, we should not expect to find, as he does, that both the objective tone and its subjective octave are effective for hearing.[29] Wrightson seems to assert that for the tone *m* the cochlea receives impulses corresponding both with *m* and with 2*m*. Moreover, the subject is expected to hear both *m* and 2*m* together; at least both *m* and 2*m* are held to be simultaneously effective in the production of combinational tones. This argument is untenable both in fact and in logic: one does not hear in a pure tone its octave, and one does not eat one's cake and still have it. If the cochlear determinant of tones is doubled, then it can not still be single.

9. With the subjective octave goes Wrightson's explanation of all differential tones but the first.[30] One of his differential tones, $(2m—n)$, does not exist, to our knowledge, for any other theory. The first differential is supposedly grounded in the nature of the compounded curves; but we have seen that the preliminary discussion indicated a differential tone an octave too low, and that subsequent discussion was based upon an improper unit. Hence there is as yet little promise of a revised theory of differential tones.

10. The summational tone is in no better case. Wrightson does not condition it upon the subjective octave, though he might have done so had he allowed differential tones of the second order.[31] The summational tone goes the way of the first differential tone; the arguments in its favor that we have noted in our first and second paragraphs are not convincing. Even were they final, they would prove embarrassing; since they would indicate that the summational tone is a prominent component of all fusions; whereas, introspectively, the summational tone is weak. Wrightson concludes with an attempt at

[28] P. 143.
[29] Pp. 147 f.
[30] Pp. 148 f.
[31] *Cf.* W. Wundt: *Physiol. Psych.*, i., 1893, 465.

diagrammatic establishment of the summational tone;[32] but he fails here for the reason that we have given, and for another reason that we must now mention.

11. The final account of the mode of treatment of the cochlear pressure-curves[33] raises new queries. Wrightson obtains a cochlear curve for a compound of two tones by adding algebraically the cochlear curves of both tones. The result is not what we should expect (*cf.* Plate VIII); since the sum of the cochlear curves of the separate tones is not what the cochlear curve of the sum of the separate tones would seem to be. Presumably, since compounding actually occurs in the air, we should first compound the sine curves, and then find a cochlear function. Be this as it may, Wrightson now proceeds by the cut-and-try method to lay off between impulse points the wave-lengths of the separate generators, of the differential tones, and of the summational tone; and *he takes as impulse points the crests, hollows, and crossing-points of the cochlear wave.* Up to this point we had thought that the reason for calling the crests, hollows, and crossing-points of the air-wave "impulse points" is their correlation with the points of supposedly zero-pressure of the doubled cochlear wave. Now we are suddenly confronted in a simple wave with *eight* impulse points; for in a single period the cochlear wave can have four crossing-points, two hollows, and two crests. Still, however, this gratuitous doubling of the number of impulse points results in graphic coincidences! It is strange that the theory, if we take the diagrams at their face value, should thus insist on proving itself. Moreover, there is something disconcerting about a theory that will come right on any assumption. We ask in bewilderment *what* it is that the theory theorizes,—whether the facts that it is supposed to be concerned with, or some irrelevant character of graphic forms.

It is a pleasure to turn from criticism to appreciation. And if the summary statement of our debt to Wrightson is shorter than the catalogue of objections to his theory, that is only because it may be set down without comment or argument. The points to which we desire to call attention lie for the most part at the level of observation,—naturally, since the conclusions at the higher level of the theory are dependent upon reasoning to which we have been obliged to take exception.

1. We must note in the first place Wrightson's discovery

[32] Pp. 151 f. and Plate IX.
[33] Pp. 150-152 and Plates VIII, IX.

that the compounding of two periodic V-shaped wave-forms of frequencies x and y gives a compound curve of $(x+y)$ and of $(x-y)$ intermediate and approximately equidistant crossing-points; and that this characteristic of compounding is independent of the phase-relation of the components.[34] Wrightson apparently made his discovery by an inspection of plotted samples, and without having the ultimate form of his theory in mind. The cases presented in Plate I are convincing samples: there seems to be no reason why the validity of the generalization should be doubted. It would be interesting to work over some of the cases accurately, by analytics or graphics, in order to determine the mean variation of the points from exact periodicity and thus to arrive at a definition of the approximation.[35] The meaning of the relation, as we have seen, is not obvious; but it is a novel idea that compound waves may be understood by a study of the occurrence of critical points within them.

2. Wrightson makes a similar discovery in the compounded sine curves, plotted by the ohmograph,[36] the invention of which is in itself a contribution to acoustics. If the hollows, crests, and points of crossing are taken as critical points, the distribution of these critical points shows simple relationships to the component wave-lengths and to the wave-lengths corresponding with the sum and difference of the component frequencies.[37] Again we are reminded of the possibility that we may solve the problem of hearing by reference to a periodic recurrence of critical points, rather than by reference to the total wave-form or the wave-form of the components. Here the analogy of the siren, where the ratio of size of hole to space between holes (wave-form) is much less important than the rate of recurrence, is peculiarly suggestive.[38]

3. The conception of the conditioning of tone on the periodic recurrence of momentary impulses raises the question of the effect of aperiodic impulses. Wrightson finds critical points that are 'aperiodic,' except that they recur at long intervals; for a wave-form compounded of two commensurable components must eventually repeat itself and therefore every point within itself. He regards these additional points of long period as the correlates of noise,[39] and remarks that the

[34] Pp. 12-17 and Plate I.
[35] *Cf.* p. 30.
[36] Pp. 24 f. and Plate III.
[37] Pp. 26-30, 60-77.
[38] Pp. 6-8.
[39] P. 35.

poorer fusions, where the 'aperiodic' points occur more frequently, are the noisier. It is true that the poorer fusions have upon them a greater roughness or *vibrato,* and it is possible that Wrightson provides a key to this introspective fact. If one can hear in a major seventh two tones *and* a *vibrato,* any theory that provides three elements in the compound wave is so far illuminating. We may point out, further, that Wrightson's characterization of the 'aperiodic' recurrences as noise agrees with Jaensch's view, and that both Jaensch's and Wrightson's noises are periodic in a long period.[40]

As with the graphics of wave-form, so with the mechanics of the ear, Wrightson accomplishes more at the level of fact than at that of interpretation. His text and Keith's appendix are full of descriptive statements concerning the structure and mechanics of the middle and internal ears that are necessarily pertinent to any theory and are themselves the material of science. They can not all be listed separately, but we may indicate their general nature.

4. The kinematic analysis of the middle ear, and its conception as a mechanical system slung between opposing elastic tensions and operating under Hooke's law, at once provide valuable data regarding the actual dimensions and the probable displacements of parts.

5. The analysis of the inner ear is similarly informative. It is of exceptional value that we should know the hinge-like motion of the stapes,[41] the dimensions of the cochlea, the relative velocities of the lymph at various points, the size and relations of the various members of the organ of Corti at different regions of the cochlea, the probable displacement of the basilar membrane, the hinging of the arch of Corti, the relation of the tectorial membrane to the hairs, the kinematics of the organ of Corti that translates basilar displacement into transverse bending of the hairs,[42], the approximate amount of this displacement,[43] the elastic resistance of the tectorial membrane,[44] the lack of correspondence between the hair cells, the tectorial fibres and basilar fibres,[45] the peculiar properties of the vascular body,[46] and the other details of like

[40] Wrightson, pp. 21 f.; E. R. Jaensch, *Zeitschr. f. Sinnesphysiol.* **47**, 1913, 256 ff.
[41] Pp. 42 ff.
[42] Pp. 95-127.
[43] Pp. 189 ff.
[44] P. 209.
[45] P. 247.
[46] Pp. 181 f.

nature. The study of the inner ear of the sparrow shows that certain parts of the human ear are in all probability inessential to a discriminative hearing of tones.[47] The suggestion that the broadening of the basilar membrane toward the tip of the cochlea is a compensation for its less favorable situation is also worthy of notice.

All these facts are of great value, even though they have not as yet eventuated in a tenable theory. They belong to the observational half of science, and the primary half. If we had had more of them and fewer theories, we should know more; or rather, if we had had more of them, we should have fewer theories. Wrightson has given us no new theory, that shall "win the assent and support of physiologists, psychologists, physicists and musicians;" so far as that goes, the Helmholtz theory may continue to rest as comfortably as its intrinsic condition permits. But he has given us more facts than we have had for many a day.[48] And he has thus begun his scientific work where it should be begun,—with observation.

[47] P. 228.

[48] Let it not be forgotten that Wrightson, when he lacked facts, sought out a competent preparator to make for him three series of microscopical sections of the human ear. See Appendix, p. 157.

ON THE NON-VISUAL PERCEPTION OF THE LENGTH OF LIFTED RODS[1]

By Louis B. Hoisington

TABLE OF CONTENTS

I. Problem

This study has been undertaken as a preliminary to the study of what is known as 'eccentric projection.' We have sought to discover and to analyze the perception of length which arises when light rods, held horizontally, are lifted a moderate distance in the vertical dimension. We have also been successful in synthetizing this perception of length from the data of analysis. We believe that the ground is thus prepared, logically and psychologically, for an attack upon the more complex perception which appears when a rod, held in the hand, is brought into contact with wall or floor.[2]

II. Observers

The observers were Mr. P. Cavanaugh (Ca), Miss C. Comstock (Co), Dr. C. L. Friedline (F), Dr. J. M. Gleason (G), the writer (H), Mr.

[1] From the Psychological Laboratory of Cornell University.

[2] Since the references to this type of experience are concerned almost exclusively with 'eccentric projection,' we omit an historical review. See, however, H. Lotze, *Microcosmus* (tr. E. Hamilton and E. E. C. Jones), I, 1885, 586 ff.; esp. on the perception of added height due to head and foot coverings, 592 f.

H. S. Liddell (L), Mr. E. S. Neal (N), Miss A. K. Sullivan (S), and Dr. M. J. Zigler (Z).[3] All were graduate students or instructors in the department of psychology. They varied considerably in training: Ca and N were unpractised; Co, L, S and Z were but moderately experienced; F, G and H were trained observers.

H and Z served throughout; F, G and N during the first half of the work; L and S during the second half; Ca and Co during the last quarter only. The observers, except H, worked without knowledge of problem or conditions. The plan of the investigation called for observation under a long series of but slightly varying conditions, and thus made heavy demands upon the patience and skill of the observers. The demands, as the results show, were adequately met.

III. Procedure and Apparatus

The method of procedure was twofold: quantitative, by the method of Constant Stimulus Differences, and qualitative, by way of introspective report. In the method of constant stimulus differences, following Urban, we used five comparative stimuli: objective equality, and two steps above and below; except in one check-series, where all rods were objectively equal.

The experimental work divides itself into four principal sections—A, B, C and F—in accordance with the nature of the rods used or the end sought. In section A (10 series) we used rods, planed from seasoned poplar wood, one-half inch in diameter, which averaged 1 gr. in weight per 2.85 cm. of length. The handles, turned from white pine,, were quite heavy relatively to the weight of the rods. In section B (7 series) the rods, planed from seasoned oak, were three-quarters of an inch in diameter, and averaged 1 gr. in weight to every 0.52 cm. of length. The handles, also turned from white pine, were both absolutely and relatively much lighter than those used in section A. In section C (1 series) we aroused the experience of length without recourse to rods, synthetically. In section F (6 series) we sought to determine more exactly the effective factors in the perception of the length of lifted rods. To this end we used the lighter rods with a new set of lighter handles. We added weights to the rods, to make them conform to certain mathematical requirements; hence they were about double the weight of the rods of section A. We give the data for the physical properties of the rods in Table I.[4]

This table shows three characteristic ways in which rods may differ: length, weight and center of mass. All three moments may differ for every rod in a set; or any one moment may be kept constant for every rod, the other two varying; or two may be constant for every rod in the set, the other varying. We thus obtain seven different combinations. Use of the same combinations with the light and the heavy rods gave fourteen series; the first seven of section A and all

[3]Thanks are due to Miss M. Kincaid and to Dr. C. W. Bock, who acted as experimenters for H.

[4] The handles for section A were 15.4 cm. long and weighed 70 gr.; for sections B and D, they were 15 cm. long and weighed 30 gr.; for sections E and F they were 10 cm. long and weighed 30 gr. The centers of mass in series A III lay 35, 33.5, 31, 29.5, and 27 mm. back from the front end of the handle.

of section B.[5] In addition, we ran a control-series (A VIII), in which all the rods were equal; a practice-series (A IX), which was

TABLE I.

Series	Total Length of Rod and Handle	Center of Mass from Handle	Weight of Rod and Handle
A I	74, 79.7, 85.4, 91.1, 96.8 cm.	11, 23, 35, 47, 59 mm.	96, 98, 100, 102, 104 gr.
A II	Same as A I.	Same as A I.	All 104 gr.
A III	All 22.9 cm.	See foot note	Same as A I.
A IV	Same as A I.	All 59 mm.	Same as A I.
A V	Same as A I.	All 59 mm.	All 104 gr.
A VI	All 85.5 cm.	Same as A I.	All 100 gr.
A VII	All 85.5 cm.	All 35 mm.	Same as A I.
A IX	Same as A I.	Same as A I.	Same as A I.
A X	Same as A I.	Same as A I.	Same as A I.
B I	85, 87, 89, 91, 93 cm.	222, 233, 244. 255, 266 mm.	192, 196, 200, 204, 208 gr.
B II	Same as B I.	Same as B I	All 208 gr.
B IIIa	All 25.5 cm.	21, 23, 25, 27, 29 mm.	Same as B I.
B IIIb	All 89 cm.	Same as B I.	Same as B I.
B IV	Same as B I.	All 244 mm.	Same as B I.
B V	Same as B I.	All 244 mm.	All 208 gr.
B VI	All 89 cm.	Same as B I.	All 200 gr.
B VII	All 89 cm.	All 244 mm.	Same as B I.
C I	Synthetic Series.		
D I	Same as B I in all respects. Wt. judgments.		
E I	All 82.5 cm.	All 250 mm.	60, 80, 100, 120, 140 gr.
F I	All 82.5 cm.	74, 76.9, 80, 83.2, 86.7 mm.	208, 204, 200, 196, 192 gr.
F II	All 82.5 cm.	206, 222, 240, 261, 284 mm.	111.6, 105.85, 100, 94, 87.9
F III	All 82.5 cm.	Same as F II.	116, 108, 100, 92.2, 87.9 gr.
F IV	All 82.5 cm.	202, 220, 240, 262, 285 mm.	All 100 gr.
F V	All 82.5 cm.	Same as F IV.	113.5, 106.6, 100, 94, 87.6
F VI	All 82.5 cm.	Same as F IV.	119, 109, 100, 91.7, 84.2 gr.

[5]The 7 series of section B duplicate the corresponding series of section A in the ordering of the three physical moments.

a repetition of A I, at the end of the year's work; a passive series (A X), in which the support of the rod, after *O* had grasped the handle loosely, fell away gently; the synthetic series (C I); a weight-series, in which the *O*'s judged the comparative weights of the rods; two short series in which a wooden 'spoon,' securely bound to the arm, served as artificial thumb or forefinger to take up the 'kick' or the 'pressure' as the case demanded; and a series (E I) in which center of mass and length were the same for every rod, but weight varied very considerably. Lastly, we ran a somewhat irregular lot of series (Section F) in the hope, as before stated, of finding some further cue to the analysis of the perception. We worked out the weight and the center of mass that would give us a desired ratio of 'kick' to 'pressure,' and increased this ratio by relatively equal amounts for every one of the five variable rods (F I and II). In this way we hoped to find an optimal setting of the rods—i. e., that combination of physical moments which should give the smallest D. L., or whatever corresponds with that determination. We also (F III, V and VI) placed weights on the rods at such points that the 'kick' or the 'pressure' remained constant for every rod in the set.[6] In F III the steps were relatively equal increments of the variable pressure. Finally (F IV, V and VI) we took for our steps relatively equal increments of center of mass. In sections A, B, D and E the steps were absolutely equal units of weight, of length or of center of mass. There were, then, one, two or three sets of equal but disparate increments, acocrding to the number of variable factors for the given set of rods.

In an experimental group 20 pairs of rods were presented in a haphazard order, in which every one of the variables occurred an equal number of times. On the completion of a group, *O* rested for a few minutes. The number of groups completed in an hour of observation varied from three to five. Fifty judgments in all were made with every variable in every series, except the first in A and B, when for the sake of practice the number was increased to 100.

The time elapsing between presentations of the rods was, at first, regulated by a soundless metronome. The use of the metronome was discontinued after *E* had become practised. Six seconds intervened between the giving of the judgment and the presentation of the first rod of the next pair. We found that, with this interval, the tendency for the impression of the last rod of a preceding pair to carry over to the first of the succeeding pair was almost negligible. The second rod was taken up by *O* as soon as he was able after putting the first rod down. The time elapsing between the lifting of the second rod and the giving of the judgment varied considerably. The *O*'s were, of course, urged to give immediate judgments; if they reported an attitude of doubt or uncertainty, the pair was presented again at a later time. The time for lifting the rods and making the judgment, when the report was immediately rendered, averaged about 10 sec. Some *O*'s, however, were slow and deliberate in the act of lifting, while others made the lift very quickly.

We employed the two time-orders with equal frequency but in no regular sequence, so that any effects of warming-up or of practice

[6]Throughout this study we use the term 'kick' to designate the pressure-sensation at the base of the thumb stimulated by the up-thrust of the back end of the handle, and the term 'pressure' to designate the sensation in the forefinger stimulated by the down-pull of the outer end of the rod.

were equally distributed. *O* was not informed as to the time-order in any series. We found that, so long as he did not take a positive set or attitude toward time-order, there was complete uniformity in results. Some *O*'s showed a tendency to judge the second rod as 'longer' or as 'shorter' with a resulting difference in distribution for the two time-orders.[7] The results given are the averages for the two time-orders.

Since it was advisable that *O* should take all rods from the same place, we made a long narrow frame which slid back and forth with but little noise over round metal bearings and between greased guides, and placed it on a low table. We cut shallow notches with sloping sides 6 in. apart in the sidepieces of the frame, and faced the top of the notches with thin felt. The rods, placed in the notches, lay across the frame in the horizontal position with the handles projecting over the edge of the table toward *O*. Thus *O* took all rods from the same position, relatively to himself, since a small movement of the frame, made by *E*, brought the desired rod directly in front of him. We were able by means of this device to avoid a space-error.

The *O*'s sat blindfold during the observations. A counterbalanced screen left only the handles of the rods exposed. This screen remained down, so that the rods were completely covered, whenever the blind was removed. *E* marked the position of the chair for every *O*, so that he should sit, always with the chair in the same position, at full arm's length from the handles. *O* deposited every rod, as lifted, in a cloth-lined tray placed just above and to the right of the rod when in position to be taken up. This tray was shorter than the rods, except for series A III and B IIIa; hence *O* could get no cue to length when he put the rod down.

For the regular quantitative series the written instructions handed to the observers were: "You will be given two rods in succession. You are to raise them in the horizontal position, using a whole-arm movement and a loose grip upon the handle. You are to judge the length of the second rod in terms of the first; that is, you are to judge the second rod as longer than, equal to, or shorter than the first." For the introspective reports we took separate short series at the end of the regular groups. The additional instructions ran as follows: "You will make a full psychological report of the processes set up in the two experiences."

E's task was threefold: he shifted the sliding frame back and forth; he returned the rods from the tray to the frame; and he recorded the judgments and reports of *O*.

IV. Results

Our criterion for the adequacy of the perception of length is the ability to make comparative judgments. If the percep-

[7] On two different days, owing to a previous discussion with *E*, H assumed, without intending to do so, a time-order set. The results were comparable with those on other days if the set corresponded with the time-order given; if not, the results showed the discrepancy. In both cases *O* took the normal discriminative length-attitude at the next sitting. Following accepted practice, it would seem allowable, if *O* tends to note the time-order used, to tell him what the order is. But it appears best to say nothing about it, and to hold *O* to the attitude set up by the instructions.

tion changes in degree with variation of one or more of the physical moments of the stimuli, we shall expect a high percentage of 'shorter' judgments at the one extreme and a small percentage at the other, provided that the extremes have been well chosen. The opposite will be true of the 'longer' judgments. If, on the other hand, certain of the physical moments are not adequate stimuli to the perception, or affect it only indirectly through association and reflection, then, with these moments the sole variables, we shall expect a more irregular distribution of the comparative judgments; one with low extremes for either kind of judgment and with inversions, one with a probable excess of the 'equal' judgment. This is what we found. A certain factor (center of mass), when varied, gave typical cross-curves with low intersections; the other factors (length and weight) as variables gave flat curves with inversions and delayed responses.

The distributions of the comparative judgments for all *O*'s with the different arrangements of the physical moments (see Table I) are given in Figs. 1 to 26, where the abscissae represent the five variables, and the ordinates show the percentage that the number of judgments in each category (longer and shorter) formed of the total number of judgments given.

Length

It is clear that length, in and of itself, cannot affect the rod as a stimulus-object. It is impossible that length alone, apart from its usual connection with center of mass and weight, can affect a rod which functions as a lever. Center of mass and weight are the factors that enter into the computation of the moments of force. The pendular period of a rod is likewise dependent upon its center of mass. It is only in so far as air resists the motion of a rod that greater or lesser length, presenting a more or less extended surface, can be effective in experience. And it is out of the question that such a difference should be effective in the present experiment. In a trial synthetizing apparatus we used, without effect, a rod considerably longer than any of those of the regular series.

We, nevertheless, let length take its turn, along with the other two moments, at varying and remaining constant. The results (Figs. 5 and 15) show that, when length is the only variable, accurate judgments of difference in the length of the rods are impossible. The curves for all *O*'s are flat and irregular. These results should be compared with those obtained when all the rods in the set were equal (Fig. 8).

It must be remembered that, since weight and a center of mass out beyond the hand were present, the perception of length as such was possible and took place. The introspective reports can give us nothing new on this point.

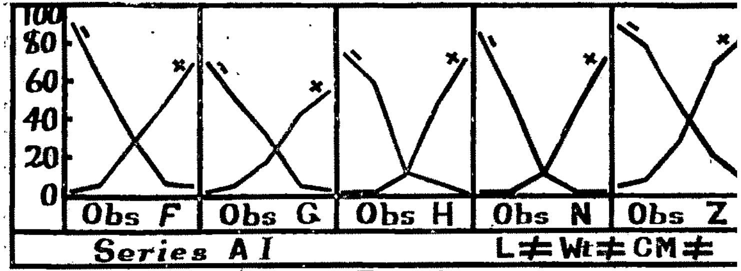
Weight

Since we have ruled out length as a contributing factor to the length-perception, we are free to examine the series in which weight and length were both variables as well as the series in which weight alone varied (see Figs. 4, 7, 14 and 17). Scrutiny of these series shows that the influence of differences of weight upon judgments of comparative length is a complicated matter.

The adjustment of the physical moments in series A IV (Fig. 4) gave a rather large per cent. of 'shorter' judgments for the lightest rod; the per cents. for Z and N are 66 and 65 respectively. All *O*'s except F gave fewer judgments of 'longer' for this series when comparing the heaviest rod with the normal; F gave the same number for 'shorter' and 'longer' at the extremes.[8] The general flatness of the curves together with the inversions shows that here was no sufficient basis for judgments of comparative length. The preponderance of the judgment 'shorter' seemed to have, in large part, a temporal basis. Two time-factors were noted by the *O*'s; the one had to do with the quickness or slowness with which the pressure came on initially, the other, with the 'liveliness' or 'deadness' of the pressure. It was the former, according to the introspections, which came in most frequently in this series. The usual muscular innervation was greater than that required for lifting the light rod. As a consequence the rod came up with surprising suddenness. In almost every such case *O* judged the rod as shorter than the standard. The lightness of the rod came, readily enough, to carry an assigned meaning of length, in the same way that the 'secondary criteria' carry meanings in the domain of visual space-perception. It is to be doubted, however, whether the length-meaning was fully realized. It is much more probable that weight was the first item of perception immediately translated into length-terms; it set off the length-meaning without the full perception of length.

We turn now to the results of series A VII (Fig. 7), where there obtains a greater practice effect (for the effect of prac-

[8] Owing to a previous injury to the hand and wrist, F lifted the rods in a different manner from the other *O*'s. Her experience differed accordingly. Wrist-torsion made up a large part of the experience.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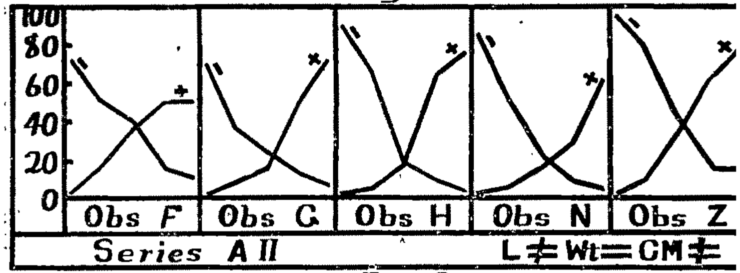
Obs F
Obs G
Obs H
Obs N
Obs Z
Series A I
L ≠ Wt ≠ CM ≠

Fig 1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Obs F
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Obs H
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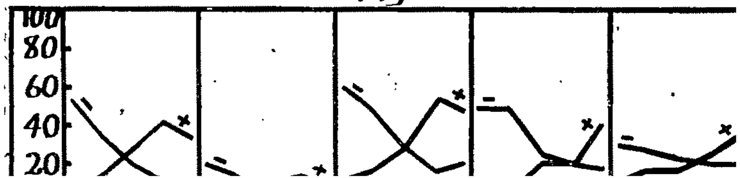
Series A II
L ≠ Wt = CM ≠

Fig 2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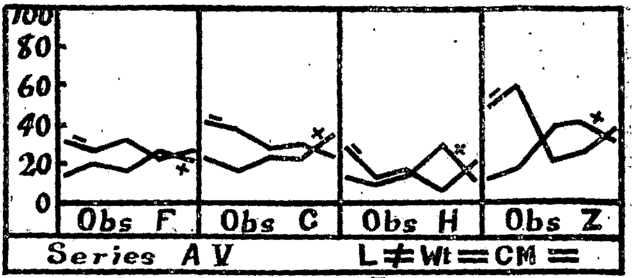


Fig 5

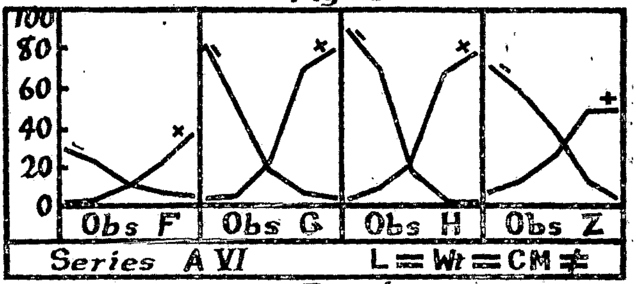


Fig 6

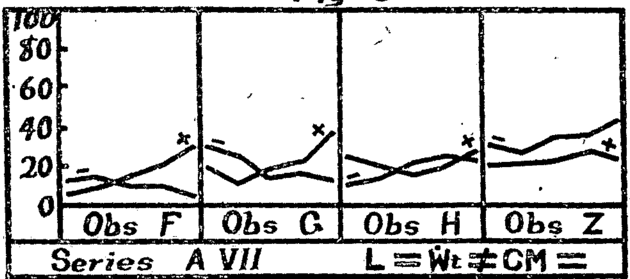


Fig 7

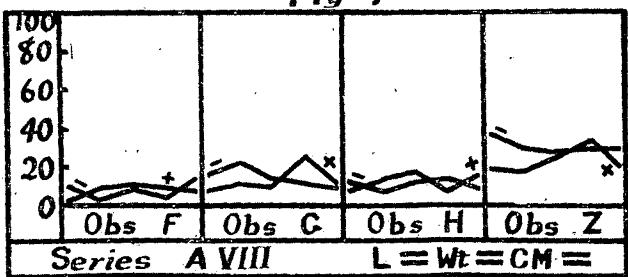


Fig 8

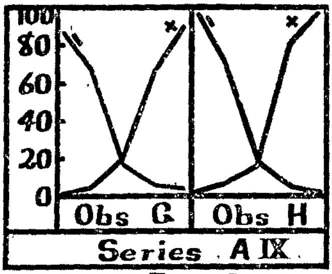
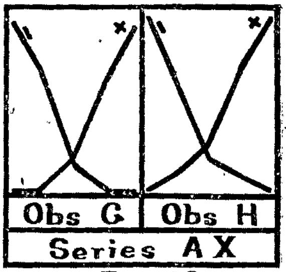


Fig 9



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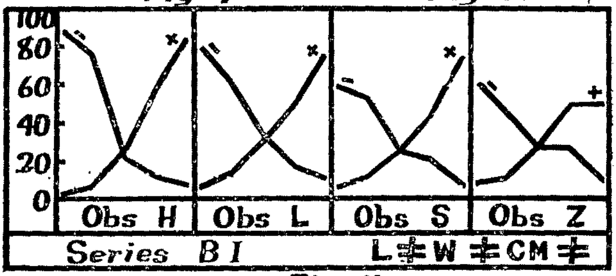

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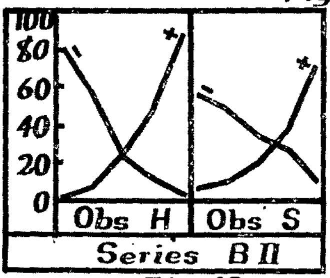

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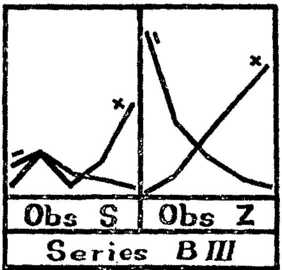

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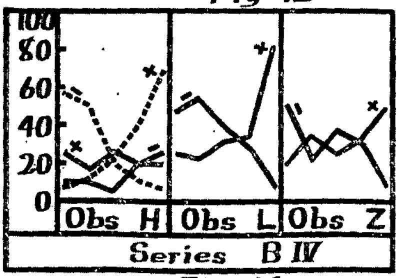

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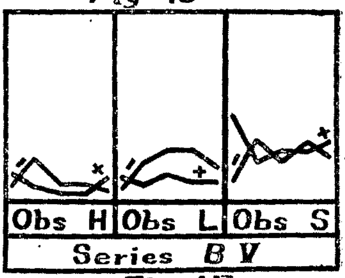

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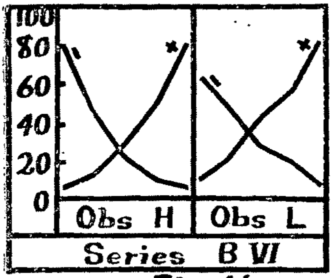
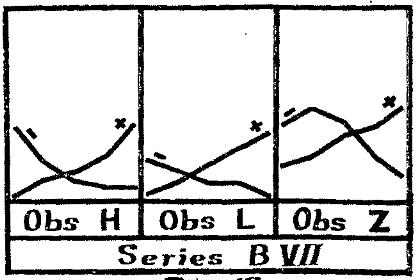


Fig 16



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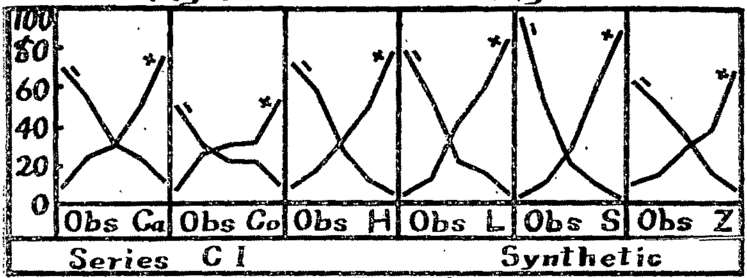

Fig 18

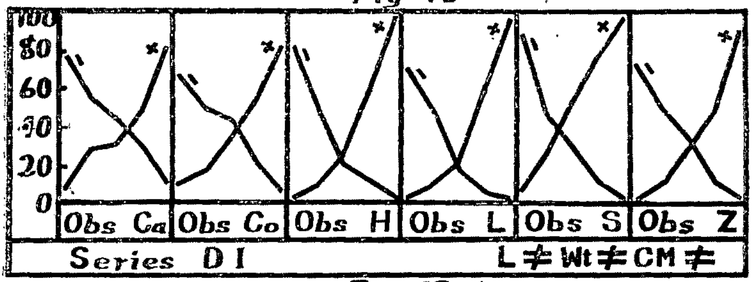

Fig 19

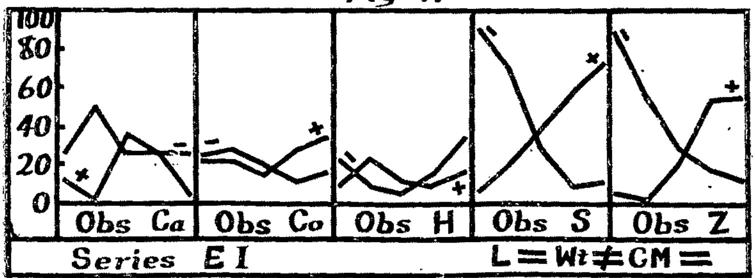

Fig 20

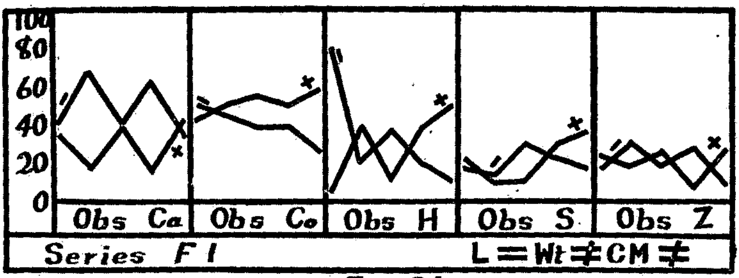

Fig 21

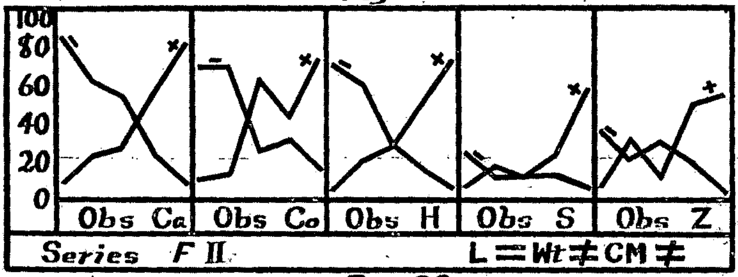

Fig 22

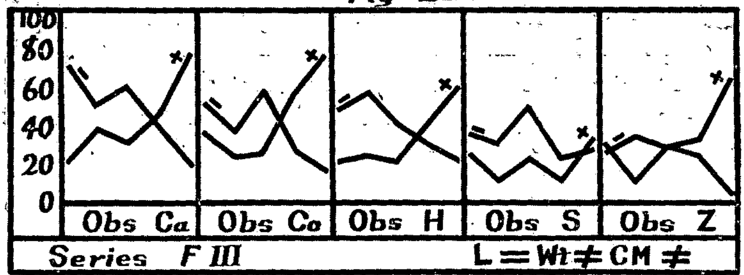

Fig 23

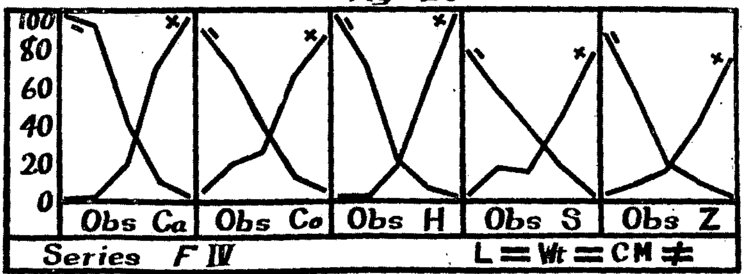

Fig 24

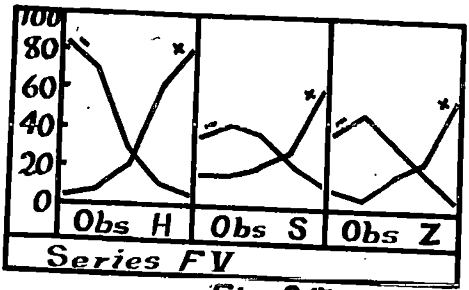

Fig 25

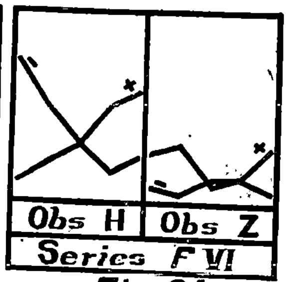

Fig 26

tice cf. Figs. 1 and 9) and where weight is the only incremented factor. The curves are flat; and there are inversions of the first order in every one save the curve for the 'longer' judgments given by F. It is obvious that there were, in this series, no sufficient sensory data for the differentiation of perceptions of length; the weight-increments, which were the same as those used in the preceding series, were not vital for the experience. Hence they cannot be held responsible for the results obtained earlier.

In spite of these curves, however, we believe that it would be a mistake summarily to dismiss the moment of weight as bearing no relation to length non-visually perceived. In Fig. 14, with weight and length varying, and in Fig. 17, with weight alone receiving an increment and a decrement, we have curves showing a tendency toward correlation between objective weight and perception of length; the lighter rods come to the *O*'s as 'short,' the heavy as 'long.' The tendency is especially striking for H in both series and for L with the heaviest rod in series B IV.[9]

This relationship finds its explanation in a gradual shift of attitude on the part of the *O*'s. They came, by degrees, to observe a single aspect, the intensive change, in the total complex (since this was the only variable present), and to report differences of intensity as differences of length. They were, naturally, determined in the direction of finding a difference; the judgment 'equal' did not mean, in many cases, the perception of equality, but the perception of not-different. It may be, too, that the introduction of the introspective series, necessitating the direction of attention upon the sensory experience, made for the translation of sensory cues into the meaning of length. We believe that, whether these series had been introduced or not, the *O*'s would have sought out some sensory cue within the complex as a means for making comparisons. Such a cue could never be adequate to the full perception of length, since this has as its primary condition not a simple attribute, inhering in a single sensation, but a ratio of intensities.

In this dilemma of having to make a judgment of difference, under the determination for differences of length, upon an inadequate sensory basis, the *O*'s seized upon the outstanding feature in the shifting complex mass, and forced that into service. Instead of apprehending the two intensities in rela-

[9]The two curves shown for H in Fig. 14 represent an earlier (dotted line) and later (full line) performance of these experiments. The change of result with shift of attitude is noteworthy.

tion, which is impossible except as they integrate in the perception of length, the *O*'s took the intensity of the 'kick' or of the 'pressure,' absolutely or in relation, as immediate data for judgment; the intensity of either or both of these pressures is affected by a change in weight as well as by a change in center of mass. The above results, accordingly, find an explanation which at the same time makes clear the true part played by weight in the judgment of length.

This shift of attitude is precisely what we might expect to take place; it is in keeping with all that we know concerning the 'natural' life-history of perceptions. The tendency is that, sooner or later, some aspect of a complex experience comes to stand for the whole; that a fragment, tag or remnant of the full sensory complement is effective to arouse and carry the meaning, fully maintained, that once accrued to the total process-complex.

But there is other evidence against the case for weight. In the experiments in which artificial wooden 'spoons' were used to receive the 'kick' in the one case and the 'pressure' in the other, the differences in the weights of the rods were not sufficient to call forth differences in the perception of length; in fact, the perception of length did not appear at all. And in series A III, where the center of mass was very near the forefinger (15-25 mm.) and varied by increments of only 1.5 mm., the perception of length was frequently lacking; the distributions of 'shorter' and 'longer' judgments (Fig. 3) shows flat curves with inversions of the first order. The more regular distributions of F and H are explicable.

F's once injured wrist necessitated a peculiar manner of taking the rods, and a dependence upon wrist-torsion for making the judgments. With a rod set as a lever in the hand, where the forefinger and the base of the thumb act as fulcrum and weight respectively, the weight or force is adequate to produce a torque in the wrist. A failure of F to render judgments of difference in this case would have proved perplexing; just as her objectively poorer results in A II were to be expected. H, unable for the most part to get the perception of length and, when it was present, unable to make judgments of difference, resorted to an individual way of taking the handles. Instead of employing the whole hand, he took the handle in such a way that in front it stimulated the forefinger alone, and at the back a very small area at the base of the thumb. By thus limiting the stimulated parts with the 'kick' and the 'pressure' (so to say) concentrated, he was enabled to make judgments. It is clear that one of two things

must have happened; either H singled out for observation the intensive changes of a single sensation; or the enhancing effect of the smaller area made smaller total-complex differences capable of carrying meanings otherwise unattainable. The former alternative accords better with the known facts. Although, therefore, H was set to report differences of length, yet by a process of substitution the inequality of intensity of the single sensation in the total complex came to touch off, directly enough for judgment, the meaning of difference of length.

Finally, series F I, F II, F III, F V and F VI demand a word of explanation. In these series the heaviest rod had its center of mass least far out and conversely, so that the two factors, weight and center of mass, ran counter to each other; they stood in an inverse ratio for the five variable rods taken in order. This result was secured by an addition to, and shifting of, the weight of the rods, in order to make them conform to certain requirements. In each one of these series either the 'kick' or the 'pressure' was the same for every rod, while the other factor increased in amount from rod number one to rod number five; the increments in series F I and F II yielded relatively equal decrements in the ratio of 'kick' to 'pressure;' in series F III gave relatively equal increments in the pressure itself; and in series F V and F VI gave relatively equal increments between the center of mass and the hand.[10] The results show marked differences between *O*'s: for some, the discrepancies of weight and center of mass cut across one another, so that the effect of the one practically nullified that of the other; for others, the differences of weight determined the judgments of length; for still others, the judgments of length reflected but slightly the opposing differences of weight. The same contradiction in results came out in series E, where we have very large increments of weight but equal centers of mass. Co and H gave hardly anything but equal judgments; S and Z gave judgments which correlate highly with weight. The fact that the judgments of two *O*'s do not correlate with weight is conclusive; the two perceptions are of different kind and, under constancy of determination, the one modifies the other but slightly even though they are at variance. The difference is that between an experience in

[10]In making our computations, we disregarded the facts that the pressure falls upon an area and not upon a point, and that the difference in the size of the hands of the observers somewhat changed the absolute values.

which a cue touches off the meaning and one in which a perception comes to full realization.

The following reports[11] taken from a much larger number illustrate *O*'s experience when set to judge differences of length by stimuli varying either in weight only or in weight and length. They supply twofold evidence for the position that differences of weight gave the perception of differences of intensity, and did not give rise to a true perception of difference of length. They show, on the one hand, that differences of weight were noted with contrary judgments of length; and, on the other hand, that differential judgments of length were touched off by differences of perceived weight,—the point being that the *O*'s could say that the judgment was one of difference of weight and not one of difference of length. They also afford proof of the statement that the arrangement of pressures within the pattern was identical for rods of different weight, the only difference being that of absolute intensity. The *O*'s evidently took the attitude of reporting the experiences upon which they based their judgment, although the instructions do not call for such an attitude. The reports are not, in all cases, rendered in strictly attributive terms; there can, however, be no mistaking their import.

Series A III: (3—5+). "There was present this time the notion of something out there beyond the handle. Can make weight-judgment always. The difference from stimulus to stimulus seems to be one of degree rather than one of pattern. This time a foundationless inference on basis of weight" (G). (3—5=) "Perception of greater weight with the second rather obscure. The clear thing was the pressure distribution in the hand, which was the same in both cases" (G). (5—3=) "Pressure-pattern same in both cases. Unequal intensity of pressure-sensations from front and back of hand give idea of length" (N). (3—1=) "First heavier. Idea of length from pressure on back of hand and pull on fingers. Second about the same degree of unbalance but lesser weight. The relation of the back and front pressures was the same in both cases, although the intensity was less in the second for both pressures" (N). (3—4+) "Greater pull in

[11]The experimental series from which the reports are taken is given at the top of each group. The numbers before the quotations indicate the rods used for comparison. For convenience, the shortest rod, or the one with the nearest center of mass or, in case they are of the same length and have the same center of mass, the lightest rod is designated as rod number one, the others coming in ascending order. Rod number three is equal, physically, to the standard. From these numbers and the table of physical moments, it is easy to determine the physical relations between any two rods involved in the reported comparison, since the order of succession in that table is the same. The judgment rendered in every case is shown by the sign +, = or —; + standing for longer, = for equal, — for shorter. These legends are used throughout.

wrist and greater pressure on fingers. End of handle presses up against palm of hand and sets up pull in wrist" (F). (5—3—) "First was heavier; more intense pressure on fingers. Heaviness gave length. Pull on end gave length" (F). (3—1=) "Pressure on fingers and pull on wrist same" (F).

It is to be noted that in these last judgments and in all others of the same kind *O* based his judgment upon the absolute intensity of a single pressure, or of both pressures, independently of their relation.

(3—5—) "I first perceived weight. Sole cue for length was pressure sensations of hand, especially forefinger. Aware of weight from muscular sensations. Attention to sensations from hand gave definite length. With second rod, I was again aware of heaviness at the end" (Z). (3—5+) "Sometimes there is intervention of the weight-factor. I am apt to assign greater length to heavier rods. By closer concentration on the pressures in the hand and especially on the fingers I can usually, on the basis of this criterion, judge the length and hence differentiate between the two factors" (Z). (3—2+) "With first, just bare pressure-complex carried over in memory-after-image. There was a little throat-kinaesthesis that meant medium. With the second experience, the pressure was the clearest part. It was perceived both qualitatively and quantitatively, but the quantitative aspect was prominent and set off the reaction *longer*" (H).

Here again we see the influence of the intensive factor taken in an absolute way. The same thing is true of the next record. We found, in support of Meumann on the comparison of time intervals, that the pattern of consciousness with the comparison stimulus differed in certain essential respects from the pattern with the normal stimulus. The latter usually resulted in a full realization of the perception of length; all the sensory data came forward; the former resulted in a very scanty perception of length, a bare meaning of more or less; the apparatus, set for giving length, responded to any cue.

(3—3+) "Pressure complex the clear thing in the first experience. With the second, the pressure at the two points was immediately perceived as greater and the response came" (H).

Series A IV: (3—1=) "I got length and a notion of rod from both. Second was heavier. Both easy balance. The pattern was the same but pressure more intense in the second" (G). (3—3=) "Got the notion of long rod from the first. Fluctuating pressure. Second less fluctuating but more intense. The two sorts of criteria give the same notion if one is not critical" (G). (1—3=) "No difference in unbalance but the second was heavier" (N). (3—5=) "First was slightly heavier. A definite idea that length was there, but can't give a comparative judgment" (N). (3—5+) "Little heavier pressure on fingers and pull in wrist a little greater with the second" (F). (3—2=) "Pressure on fingers and pull in wrist the same" (F). (3—4=) "With the first rod, I was first aware of a certain weight. Then attention was drawn to the sensations in the hand and I got a definite notion of length. It appears that I am always baffled by the determination of length in distinction to that of weight. If I do not take care, I confuse a heavier rod as being longer. Both weight and

length seemed to be determined by pressures in hand" (Z). (3—2=) "The first gave live contact-pressure with deeper dull pressure in forefinger; lively pressure of less intensity on base of thumb. Second experience less intense all round. Perception of length the same" (H).

The next reports also show both that there is a tendency for judgments of difference of length to be touched off by differences of absolute intensity, and also just the contrary: that differences of absolute intensity are ineffective to qualify the perception of length. As we have already remarked, the instruction to report differences of length, even when there were none, laid a heavy burden upon the *O*'s.

Series A VII: (3—1=) "Barest experiences. Just the same. Perception of an evenly balanced rod. When I report 'same' it is the sensory experience and not length that is the basis of judgment" (G). (3—3=) "With the first, a notion of length. Spatial pressure pattern. Second gave notion of heavy rod. Pressure perceived as weight and not as length" (G). (3—5=) "Nothing there to enable me to make the comparative length-judgment. Both uniform, static, dead. Second more intense, but did not carry meaning of length" (G). (3—2=) "With the first rod I had an immediate perception of length. This was clear. I got nothing, at first, with the second but weight" (Z). (3—1—) "First rod gave an immediate clear notion of length. With the second rod I was at first struck by considerably greater weight. Then in verbal-motor terms; 'which is longer?' The extent was limited by the points of weight-concentration" (Z). (3—5=) "The pressure-patterns were the same quantitatively and qualitatively. The second was perceived as equal, the judgment being part of the perception" (H). (3—2=) "First pressure-complex in the hand gave clear perception of length, a moderately long rod. With the second experience the pressure meant weight. Pressure more intense and static in quality. There was a tendency to say shorter" (H).

In series B III, where we used rods whose lengths, measured in terms of center of mass, were in the approximate region of the lower limen for the non-visual perception of length, but whose weight-increments were the same as in preceding series, we obtained more proof of the same kind.

Series B III: (3—1 No judgment) "Pressure on first finger and palm of hand in both. No muscular strain or deeper-lying sensations that I usually got. I seemed to be waiting for a pull downward on first finger; it didn't come, and I could not give a judgment of length" (S). (3—5=) "Meaning of length did not go beyond the handle. But the handle was stretched out. Pressure on finger and palm of hand and thumb. There was a strain-sensation, but it seemed as a bare weight. It came at the same time as pressure. It was more through the hand and a little in the wrist, not on forefinger" (S).

The following, also from the series in which we used the heavy rods, bring out the same points.

Series B IV: (3—2—) "The second experience was less intense and the judgment referred back to the first" (L). (3—5+) "The two experiences did not differ except on the score of intensity. I could not localize intensity or say how it was linked up with 'longer'"

(L). (3—1—) "Experiences were precisely homologous. The first was more intense than the second. This intensity seemed to insure or lead to the judgment" (L). (3—4+) "There was a clearly marked difference in intensity of pressure sensations in the hand. The greater intensity of the second did not carry with it a judgment of length difference" (L). (3—1—) "The second experience differed in absolute intensity and this intensity-difference went directly over into length" (L).

The next report, typical of a small class, indicates the effect upon the perception of length of shift of attitude. *O*, it will be remembered, was under a double determination; that of making a psychological report upon the process-contents of the experience, and that of making a perceptual judgment.

(3—5+) "The first thing with the second rod was sensations in the hand; those at the heel a little more prominent and intense. Almost immediately I noted a 'drawing' in the fore-arm which was more intense and 'drawing' than in the former case" (Z). (3—1—) "With the first rod, the sensations from the forefinger and the heel of the hand equally clear for a moment. The notion of length was very clear. With the second experience, I perceived sensations in the heel of the hand. Fairly definite notion of length. Then sensations in the arm with the verbal-motor 'heavier.' There also came a change in the perception of length. It became shorter. They were about equal before this time" (Z).

As regards these and the next following reports, it is to be borne in mind that, as is shown by the psychophysical results, the *O*'s were *not* able to make judgments of difference of length that have a significant degree of correlation with differences of weight. Failure to take the two sets of results together and to interweave them gives a false perspective.

(3—4+) "Pressure at base of thumb clearest part of experience. This 'live' pressure was more intense in the second case. The obscure dull pressure on the front finger was the same in both" (H). (3—1—) "The judgment had a purely sensory basis. The pressure complex was less intense in the second. The judgment came automatically without length being perceived" (H).

Finally, we cite a few reports from the last series, in which weight was the only incremented factor, to show that the perceptions of weight and length are differently determined, that the primary and secondary sensory processes in the two cases differ as to their meanings.

Series B VII: (3—1+) "The second experience less intense than the first. In the first, distinct strain in the wrist spreading to hand and fore-arm. The second noted because of lack of intensity" (L). (3—5+) "Prominent thing in the first experience was lively kinaesthesis in the fore-arm. The second experience was more intense. Pressure-complex in the hand gave rise to a conflict as to 'equal' or 'longer'" (L). (3—5=) "In both, which were similar, three complexes stood at focus; pressure from finger, dull pressure from base of thumb, and clear but not filled-out sensations from the arm. The three seemed to have a common background; in the second the background was of less intensity. The three factors were equal in all

respects" (L). (3—2+) "With the first rod, I had sensations in the forefinger from the beginning. These were the most prominent part of the experience throughout. There was an immediate clear perception of length. There was a little change in the sensations from the arm. These were superficial and disconnected from the experience. With the second rod, there were first sensations in the heel of the hand. A very indefinite notion of length. Then sensations from the forefinger and the elbow came in. Sensations from forefinger clearest. Notion of length cleared up" (Z). (3—4—). "With first, sensations of equal prominence in forefinger and heel of hand. Sensations of lower clearness in fore-arm. Fairly definite notion of length. Second rod gave an immediate notion of length, mediated by sensations in the forefinger. Much less clear sensations in heel of hand" (Z). (3—1+) "Sensations from heel of hand and from fore-arm came in. Sensations from fore-arm clearest. Very definite notion of length. Then sensations in the forefinger became practically the only component. Length increased in clearness. With second, sensations in forefinger most marked but vague sensations in heel of hand and in arm. Notion of length very clear and definite" (Z). (3—5=) "First, sensations in heel of hand most marked. Sensations in arm and forefinger less clear and less intense. Not a definite notion of length. Then attention went to the sensations in the forefinger. Sensations in the arm dropped out and sensations in heel and finger became equal. With second rod, first there was verbal-motor 'heavier.' At the same time there came in marked sensations in the forefinger; lesser sensations in heel of hand and in fore-arm. Sensations in finger more intense and deep-seated than in heel of hand. Perception of length came very clearly" (Z).

Center of Mass

Since we took as our criterion for the conditions that determine the formation of the perception of length the ability, again objectively determined, to make judgments of comparative length, we must look for that factor, if any such there be, between which and the judgments we find concomitant variation. Fortunately we find such a one; the distribution of judgments, in every series where the center of mass noticeably varied, gives an X or cross with no inversions of the first order; in every one where the center of mass was constant the curves are flat and irregular. (See Figs. 1, 2, 6, 9, 10, 11, 12, 13b, and 16.)

But if center of mass plays so important a part, as a moment of stimulus, in the arousal of the perception of length, it becomes necessary to inquire more in detail as to its mode of operation. This enquiry should lead us to an understanding of the psychology of the perception, and should contribute toward its psychological analysis. As has been previously stated, a rod or instrument in the hand or a tall hat on the head acts as a simple lever or couple: if, that is, the rod is held out in the horizontal plane, or the hat is worn slightly tilted.

Lotze has pointed out that the tilting is an essential condition of the 'feeling' of enhanced height and consequent greater significance. This means, first of all, that there are two opposing forces or pressures. If we hold a rod in the vertical, there is nothing but the weight; no notion of length arises. It is only as the instrument becomes inclined that the perception of length comes in; weight without an extended center of mass never gives rise to the perception of length; though we do not at all deny that, given a center of mass 'out there,' variations in weight may set off a kind of derived judgment of difference of length.

The absolute intensity of the two impressions can not be the basis for the judgment of length; for we judge a long light rod, which produces relatively weak pressures, as longer than a short heavy bar, which produces much more intense impressions. The answer to the question: What is the experiential foundation for the perception? thus becomes clear; our process of elimination and our introspective analyses lead us to the same result: *the ratio between the 'kick' and the 'pressure.'* In a short rod, the amount of 'kick' relatively to the 'pressure' is small; in long rods, the two pressures more nearly approach each other in intensity; with every change in center of mass there takes place a change in the ratio of 'kick' to 'pressure.' A sensation-complex of two opposed pressures, more or less fused in experience, differing in intensity within certain ratio-limits, is the sensory pattern fundamental for our perception of length as given cutaneously; though sensory or imaginal elements of other kinds may touch-off or carry the meaning. Thus it may be intensity, perceived as weight or taken as bare intensity; it may be the temporal factor; it may be the direction in which the pressures are perceived as acting upon the hand; it may be the strain sensations in the hand; it may be other processes or perceptions than these which touch off the meaning of length or which determine the judgment when we make a comparison. All such secondary cues influence the comparative judgment more than the absolute; they determine differences of length under conditions that would not admit them into the absolute judgment.

A change of ratio of intensity between our two pressures, then, is at the core of the perception of difference of length; a change realized only by varying the center of mass. A variable weight brings no corresponding change in the ratio of the two pressure-intensities; it increases both in the same relative proportion; the ratio remains constant.

Without going into a detailed discussion of the sets of

curves referred to in this section,—they speak for themselves, —we turn to the consideration of some special items of work. We have already found that, with the artificial thumb and forefinger eliminating the 'kick' and the 'pressure' in turn, the perception failed. We found, however, that when we attached the board to the arm in such a way that the band, holding the board to the arm, stimulated a single small area, the sensations from this point combine with those from the forefinger to give a perception of length. This result evidently means that sensations from other and more remote parts of the body, if they are opposed and if they stand intensively within certain ratio-limits to each other, will fuse into a total to give the perception. It is for this reason that the fisherman can feel the pull of the fish on the end of his line as 'out there;' he has the perception of the length of the fishing-rod, even though he hold it over the lower and under the upper parts of his arm. He might just as well hold it over the lower and under the upper parts of his leg; the sensations yielded up by these distant points of contact give the perception as immediately as do the nearer together and more practised parts of the hand. We made no determination of the precision of judgments of length when the sources of sensation are far apart on the body, as compared with the accuracy of discrimination when the parts are near together.

One other modification must be reported. For a short time we told the *O*'s to grip the handles very tightly. With long rods of considerable weight the perception of length was not wholly lacking; the ability to make judgments of comparative length, within the limits employed, was totally nullified. Whether there is or is not a perception of length, with rods of different length, thus depends upon the strength of the grip; if the grip be strong enough, the long rod ceases to be so taken. This can mean nothing more than that the small increments or variations in pressure are too slight to change the ratio in a perceptible degree.

In Table II we give the 'pressure' and 'kick,' their increments, their ratio to each other, and the change of ratio with successive rods, for a few selected series. The series given represent almost all the different combinations of moments employed.

Mention was made in the reports from those series in which weight was the variable of an experience that, while negating the factor of weight, yielded positive testimony to the importance of the two related pressures: it was called the feeling of balance or unbalance. In the following quotations

TABLE II.

Series	Rod	Fore Pr	F.Pr. Incr.	Back Pr	B.Pr. Incr.	Ratio of B.P:F.P.	Ratio Incr.
A I	1	248.23		153.23		1:1.6306	
	2	270.20	21.97	172.20	19.97	1:1.5691	.0615
	3	292.86	22.66	192.86	20.66	1:1.5185	.0506
	4	316.20	23.34	214.20	21.34	1:1.4761	.0424
	5	340.23	24.03	236.23	22.03	1:1.4402	.0359
A II	1	268.91		164.91		1:1.6306	
	2	286.74	17.83	182.74	17.83	1:1.5691	.0615
	3	304.57	17.83	200.57	17.83	1:1.5185	.0506
	4	322.40	17.83	218.40	17.83	1:1.4761	.0424
	5	340.23	17.83	236.23	17.83	1:1.4402	.0359
A III	1	170.06		74.06		1:2.2962	
	2	175.70	5.64	77.70	3.64	1:2.2613	.0349
	3	181.43	5.73	81.43	3.73	1:2.2280	.0333
	4	187.24	5.81	85.24	3.81	1:2.1966	.0314
	5	193.14	5.90	89.14	3.90	1:2.1667	.0299
A IV	1	314.06		218.06		1:1.4402	
	2	320.60	6.54	222.60	4.54	1:1.4402	.0000
	3	327.14	6.54	227.14	4.54	1:1.4402	.0000
	4	333.68	6.54	231.68	4.54	1:1.4402	.0000
	5	340.22	6.54	236.22	4.54	1:1.4402	.0000
B VI	1	787.50		587.50		1:1.3404	
	2	815.00	27.50	615.00	27.50	1:1.3252	.0152
	3	842.50	27.50	642.50	27.50	1:1.3113	.0137
	4	870.00	27.50	670.00	27.50	1:1.2985	.0128
	5	897.50	27.50	697.50	27.50	1:1.2867	.0118
B VII	1	808.80		616.80		1:1.3113	
	2	825.65	16.85	629.65	12.85	1:1.3113	.0000
	3	842.50	16.85	642.50	12.85	1:1.3113	.0000
	4	859.35	16.85	655.35	12.85	1:1.3113	.0000
	5	876.20	16.85	668.20	12.85	1:1.3113	.0000
F IV	1	352.50		252.50		1:1.3960	
	2	375.23	22.73	275.23	22.73	1:1.3633	.0327
	3	400.00	24.77	300.00	24.77	1:1.3333	.0300
	4	427.00	27.00	327.00	27.00	1:1.3058	.0275
	5	456.43	29.43	356.43	29.43	1:1.2806	.0252
F V	1	400.00		286.53		1:1.3960	
	2	400.00	00.00	293.41	6.88	1:1.3633	.0327
	3	400.00	00.00	300.00	6.59	1:1.3333	.0300
	4	400.00	00.00	306.32	6.32	1:1.3058	.0275
	5	400.00	00.00	312.35	6.03	1:1.2806	.0252
F VI	1	418.82		300.00		1:1.3960	
	2	408.99	9.83	300.00	00.00	1:1.3633	.0327
	3	400.00	8.99	300.00	00.00	1:1.3333	.0300
	4	391.74	8.26	300.00	00.00	1:1.3058	.0275
	5	384.18	7.56	300.00	00.00	1:1.2806	.0252

taken from the series in which center of mass varied, we have still further proof of a positive character.

Series A I: (3—4+) "Greater strain on back of hand. Pressure sensations from back of hand greater in second" (F). (4—3—) "Wrist same. Strain factor from fingers to wrist same. Push down on the fingers the same. The thing seems to depend on the pull down from away out there" (F). (1—3+) "Pull in wrist greater and more of 'feeling of going down;' pressure down on thumb and two small fingers. Also pull on back of hand. Pull on back due to end of handle pressing on palm of hand" (F). (3—2+) "The strain-feeling in wrist greater for the second. Also a pull on hand, especially on front finger and back of hand. A sort of stretch" (F). (3—3=) "Pressure alone vivid. Set for pressure. Perceived quantitatively. 'Long' applied to pressure. Notion of rod did not come in. In the second experience, the pressure was the clear part and set off the judgment automatically. Rod context came afterward" (G).

The above report implies, as does many another, a difference between the 'bare length' attitude and the 'length of *rod*' or objective determination.

(3—2=) "Positive judgment set off by hand-complex and slight increase as rod pulls down. This gives meaning that rod is there. Judged on basis of pressure-complexes which were the same in both cases" (G). (3—5+) "Nothing in the first except the perception of pressure and measuring of it. This carried in memory-after-image. 'Longer' came with the second quite automatically with increased pressure. I really judged in terms of heaviness. Length-notion came after" (G). (3—1—) "Resistance less with the second; it came right up. With the first there was more pressure on the back of the hand; the second, even pressure" (N). (3—3=) "I concentrated attention on the shoulder. The shoulder gives criteria for weight. The perception of length seems to come from the pressures in the hand" (N). (3—5+) "With the second, more intense pressure on the fingers and relatively more intense on back part of hand where handle comes against it. This gives length; the length gives pressure at two points" (N). (3—3=) "It is the balance-pattern in the hand that gives notion of extent. It is in this way different from mere weight" (N). (2—3+) "In lifting the first rod there was a lightness of pressure in the hand. The pressure in the hand, with the second, was greater as well as the weight" (Z). "I think in terms of length in relation to compactness or concentration of weight. That is, if the weight is closely compact, it may cause as great a pressure with a short leverage as a more distributed weight on a longer leverage" (Z). (3—4=) "In lifting the former object, I was conscious of an object of considerable length. In lifting the latter rod, I got a notion of weight and pressure in the hand due to leverage. The latter no longer than the former though considerably heavier" (Z). (3—3=) "The clear processes were pressure-sensations from forefinger and base of hand. Judgment was made immediately on basis of comparison of these two sensory complexes. No definite notion of length in either case. Pressure-pattern, that is, the relative distribution of pressure at these two points, was the sole criterion for judgment. They were the same in both cases" (H). (3—5+) "Pressure greater on back of hand. I didn't notice much difference in the forefinger" (H) (3—5+) "Again, pressure at those two points, only, came into the experience.

In lifting the second rod, the pressure seemed to be a little bigger spatially; larger rather than more intense. Gave rise to notion of greater length; greater leverage" (H). (3—4+) "The only mental processes which enter into the judgment at all are pressure sensations from the hand, especially from forefinger and base of hand. In this case, the sensations from these two points, particularly the back, were a little more intense with the second rod. This distribution of pressure gave me the perception of length, although no particular length was assigned to either of the rods" (H).

In the above series weight, as well as center of mass, was a variable component. The following reports were made when center of mass and length were variables.

Series A II: (4—3=) "Quantitative judgment of the first. Verbal-motor, 'great.' With the second there was a tendency to call it lighter. Then the pressures came to clearness and carried the meaning 'equal.' The reaction followed" (G). (3—1—) "Pressure-complex, qualitative and spatial, in hand. There was no notion of rod. In the second, the pressure-complex was clear. Judgment came immediately. Qualitative and spatial pattern and not weight or quantity is significant" (G). (3—3=) "The clear sensations came from the hand. The pull down on the second finger, and the use of the forefinger as guide while it is barely in contact with the handle. Also pressure from the base of hand" (N). (2—3+) "Pull on skin in addition to the greater pressure and pull of the second all gave meaning of greater unbalance" (N). (3—3=) "With first rod, pressure in hand the clearest part of the experience. Length was assigned mainly in terms of pressure in hand. Apparently forefinger acts as fulcrum between the sensations in back part of hand and arm, on the one hand, and length of rod, on the other. Can't differentiate any mark by which I become aware of weight separate and apart from those whereby I assign length: *i. e.,* at the same time that I assign length there is also a definite assignment of weight. When the weight association comes in, the judgment is mediate" (Z). (3—4+) "In first, I was aware of muscular sensations in the arm, then of sensations of pressure in my hand. There was immediate definite length. With the latter rod, there were also sensations in arm and hand. It appeared that the long rod was acting as a lever over the hand. This was assigned length due to the pressure exerted in the hand by the leverage" (Z). (3—5+) "More stretch or pull in wrist. More pull in back of hand with the second" (F). (2—3+) "Little pull in wrist with the first. The pull a little greater in the second. Seemed to be the same weight and the same pressure on the finger in both. Pull in wrist as if the thing reached out longer and gave more pull in wrist" (F).

It may be recalled that the pull in the wrist for F is the sensory experience set up by the back end of the handle pressing up.

(3—1—) "With first, pressure-pattern-complex clear. Second, sensations in forefinger first. Then sensations at base of hand set off judgment 'shorter.' These were less than with the former rod" (H). (3—3=) "Clear thing in first experience was pressure in hand. With second rod pressures in hand very clear and equal to the former experience. Judgment followed immediately" (H).

Series A VI: (3—5+) "Perceived the first as fairly long rod.

Criterion simply intensity of pressure. Pressure fairly intense and a little more so away from body. Second more vivid and alive. Pressure very intense on outer finger. Perceived as 'longer.' Then notion of long dipping rod" (G). (3—1—) "Got notion of length; part of immediate perception. Then sensory experience became clear; live, fluctuating, more intense away from body. Second, perceived immediately as 'shorter.' Perfectly uniform pressure without change" (G). (3—5+) "Greater pressure on fingers and greater pull in wrist with second" (F). (3—2=) "Pressure on fingers and pull in wrist same" (F).

By this time the perception of length came quite immediately, and the *O*'s were often unable to get a vivid sensory experience or a full perceptual realization of length before the meaning itself became clear. There is, moreover, some evidence that the meaning may develop without any process-content at all becoming vivid.

(3—5+) "With first rod an immediate definite notion of length in motor terms and it remained stable throughout. With second, much greater relative pressure and in a flash seemed very much longer with verbal-motor 'longer'" (Z). (3—2=) "The first gave immediately, in motor terms, a fairly definite notion of length. Sensations in hand then became clear. With second, again all at once, upon lifting the rod, I got notion of length in motor terms. Verbal-motor 'equal.' The pressure-sensations again became clear and were equal to the former. The critical comparison or criticism of the automatically rendered judgment based on pressures in hand" (Z). (3—4+) "Judgment made on quantitative basis. Second, pressure at base of hand more intense relatively to pressure on forefinger, and the entire pressure-pattern more intense than the first. First, like well balanced rod; second good deal of pull down to it" (H). (3—1—) "In first, pressure moderate, evenly distributed. A well balanced rod. An immediate perception of length. In second, pressure was much less *in toto* and pressure at base of hand very slight. A little rod tipping up. 'Shorter' came as a part of the perception of length" (H). (3—5+) "In all these cases, the perception of length comes immediately. Sensory basis becomes clear afterward. Pressure in first that of a well balanced rod; liveness gave meaning of moderate length. With second, pressure more intense; strong pull down. Tendency after judgment to call the second heavier. Intensity of pressure at base of hand meant 'longness'" (H).

The series in which we used the heavy rods gave us reports of the same general bearing.

Series B I: (3—5+) "Pressure more on three fingers and muscular sensation in fingers. In the second, intensity of pressure on palm of hand and base of thumb. More muscular sensation in the whole hand, in the second" (S). (3—5+) "Pressure and muscular sensations greater in second. Also deep pressure-sensation in thumb and greater tension in second" (S). (3—4+) "All in hand. Pressure a little in second. Muscular complex in hand. Hand adjusts itself a little differently to different lengths" (S). (3—2—) "Muscular tension in fingers. Pressure on thumb. Second, pressure on thumb and muscular tension in fingers. Pressure less in last experience" (S). (3—1—) "Second came up easier, and it didn't drag or pull the forefinger down as a long one would" (S).

The strain-sensations set up in the hand as the organism tends to resist the two opposing pressures and to hold the rod in the horizontal position, enter now as a new secondary factor for carrying the meaning of length.

(3—2—) "The meaning, length, is placed upon the differential pressure-sensations in the hand; *i. e.*, the greater or less pressure on the forefinger *versus* that on the heel of the thumb carries meaning of length somehow, not visually, projected beyond the hand. The hand has come to mean fulcrum" (L). (3—5+). "In the first rod the pressure-complex was clear. Deep pressure was especially prominent. In the second case, the strain-sensations in the arm seemed coordinated with the pressure-sensations in the ball of the thumb" (L). (3—5+) "The handle, in this experience, was grasped less tightly, therefore the differential pressure-complex was more definitely analyzed: the pressure-complex of thumb and finger from that of the palm of hand. Torque was localized in hand. How torque functioned in connection with the pressure-complexes, I can not say. This torque is all in the hand" (L). (3—2—) "Experience centered about sensations in the hand. In the first, a clear differentiation. Light pressure and contact from thumb clear. Also sudden and intense deep pressure from palm of hand. The second experience was the same in the main, only less intense, especially on the thumb. Judgment immediate" (L). (3—1—) "Experience of the usual sort in which the judgment followed immediately. Difference that demanded attention was localized in hand and was of temporal nature. Complex of pressure and kinaesthetic sensations whose course was slower in the first experience than in the second. As to region of hand, contrast between inner side of hand just behind forefinger, and the base of the thumb" (L). (3—4+) "First rod gave clear sensations in forefinger and less clear on back part of hand. Experience in lifting second, sensations of forefinger more intense. Also more extensive. Immediately judged it as 'longer'" (Z).

It often happened, as in this case, that *O* noted the difference of intensity at a single point, say the forefinger; a fact which might seem to contradict our general conclusion. The contradiction would be real, however, only if we were in face of something other than distribution of attention; if *O* did not in some way take account of the relative as over against the absolute variation.

(3—1—) "With the first rod, I experienced, first, fairly clear sensations in the hand, especially on forefinger and at base of thumb. With the second rod, I had sensations in hand, wrist and fore-arm. Attention not demanded at any one point" (Z). (3—4+) "I first noticed, in the first experience, sensations in the index-finger which were very intense and pressed the forefinger down tensely against the wrist. There followed, soon, sensations in ball of thumb. Second rod, somewhat similar experience for the first moment. Pressure on forefinger following, down to wrist, and lesser sensations at base of hand. These accompanied by slightly greater strain-sensations through the hand." (Z). (3—1—) "Dull pressure-sensations at base of thumb especially clear in the two experiences. Much less intense with a somewhat 'bright' quality in the second" (H). (3—3=) "The pressure from the forefinger and base of hand the same in the two experiences. The

clear observation of the sensory complex in the second followed the judgment, which seemed a part of the perception" (H). (3—2—) "The pressure on the forefinger was, in every observable respect, the same for the two experiences. The pressure at the base of the thumb was less intense in the second case. Also it had the meaning of 'liveness' upon it, due to a 'bright' quality and a temporal fluctuation" (H).

Since section B in a sense duplicated section A, only a part of the *O*'s were required to perform both series. It is for this reason that we have introspections from two or three only.

Series B II: (3—4—) "Strain in skin, forefinger and hand. The second more intense, with increased pressure on the thumb" (S). (3—1—) "Pressure in palm of hand, in first. A little strain in first finger. Less strain and less pressure in the second" (S). (3—4=) "In first strain in fingers and down through palm of hand all as one sensation. Second, same in distribution and intensity" (S). (3—1—) "Strain-sensations in finger and palm of hand. Pressure-pattern at base of thumb. Second, lack of strain-sensation in hand, less intense in fingers" (S). (3—5+) "Pressure on forefinger and at base of thumb the clear processes. Less clear muscular and strain-sensations through hand. Marked increase in intensity at base of thumb; less marked increase on forefinger. The second pressure more extensive, giving rise to notion of its pulling down more" (H). (3—2—) "The two experiences were qualitatively the same. The second was a little less intense at the base of thumb. The second also less extensive. Strain-sensations through the hand the same" (H).

Series B VI: (3—4+) "Most prominent distinction between the two, and which seemed to carry meaning 'longer,' was the temporal sequence in sensations at base of thumb. In the second they rose more slowly. Temporal course in first more rapid. This may have been confused with amount of change passed through. Greater range in second; *i. e.*, the relative amount of increase of intensity was greater with the second for this particular complex" (L). (3—5=) "The two experiences as usual, except for a peculiarity in second. Began by intense pressure on thumb, and this brought at once the meaning 'longer.' But pressure decreased and then increased on forefinger. This negated the earlier intense pressure against base of thumb" (L). (3—1—) "The sensation-area from forefinger constant and in background of consciousness in both. In first, the pressure at base of thumb showed a transitional change and rose to high intensity as a block of experience lifted out. In the second, the whole thing seemed a flat pattern with only a vaguely traced boundary between pressure on forefinger and thumb" (L).

All these reports from the introspective data corroborate the findings from the psychophysical series as presented in graphical form.

Synthetic Series

The best test of any hypothesis regarding the analysis of a perception is that of reconstruction under the observed conditions. If our assumptions are well founded, then the perception of an extent beyond the hand should be aroused by any two pressures, so long as these are kept between certain limits of

ratio of intensity, of spatial relationship, and of direction. Taking these facts as our point of departure, we set out to produce the perception synthetically.

The matter of thus setting up the length-experience with no length present reduced itself finally to the problem of devising a means by which the two opposing pressures could be applied to the hand (since this was the part of the body with which we had worked) in a way that should be under perfect control. We found that when we took two short pieces of wood like the handles, and pressed the one up against the base of the thumb and the other down upon the forefinger, our *O*'s gave immediate judgments of length: in fact they were unaware, according to their own report, of the substitution of the two short pieces for the one long rod. Further preliminary trials proved that the length, as perceived, could be made 'longer' or 'shorter' by alternately increasing or decreasing the 'kick' and decreasing or increasing the 'pressure;' and that the perception broke down if *E* increased either 'kick' or 'pressure' beyond certain limits, when *O* perceived the two pressures separately as two unrelated experiences. So far, however, our pressures were not subject to rigid control.

The problem demanded two physical pressures, which were to be resisted independently, to operate in opposite directions, to be stable in duration, definite in amount, and independently variable. The first apparatus consisted of a handle, with an oval hole cut through lengthwise, slipped over a small rigidly supported rod set in the horizontal plane. A short pin, set into small holes bored through handle and rod at right angles to their principal dimension and at close intervals, served as a pivot, so that the handle had a considerable latitude for movement up and down about the pivotal point. By altering the point of pivoting, the length of the two lever arms changed inversely, while at the same time the down-pressure of the fore end and the up-kick of the back changed in an inverse ratio. Since the effective physical pressure is a function of the length of lever arm and of the weight or force applied, these pressures could be determined and varied by assignable amounts; the weight, hung at the front end of the handle, tended to actuate rotation of the handle in the hand of *O*, who gently resisted the movement. The device was a success so far as the evoking of the perception of length was concerned. It was, however, ill-adapted to the evoking of judgments of comparative length, since *O* might, by using the pivot as a fulcrum, resist all force of the weight at either the fore or the back point of contact.

Next a split handle was arranged, with the top half pivoted in front and moving upward through a small arc at the back, and the bottom half pivoted behind and moving downward; the movement was produced by weights attached to cords which ran over pulleys. The two pressures could be varied at will, and must be resisted independently. The perception of length did not invariably result, for the *O*'s were immediately aware of the division in the handle, and might allow the one half to move through a relatively large arc while they held the other from moving at all.

This apparatus was given up as a failure, and in its place we made a handle with a smooth horizontal hinge at the middle; the back end being pushed up and the fore end pulled down by easily variable and definite amounts. *O* took the handle loosely and the weights released caused the handle to dip down in front and up behind, just as a rod would behave. So long as the

handle did not 'break' at the hinge, the perception of length was definite and controllable; but a 'break' destroyed the perception, since the two pressures were no longer 'in line.'

Finally we hit upon a simple and satisfactory arrangement; a handle suspended on a cord attached at the front end. Fastened to this handle were two weights; the one, fixed to the lower side at the front end, pulled down; the other, fastened to the back and by means of a cord run over a pair of pulleys, pulled up. There remained one disturbing factor; the handle was unstable; it seemed unattached. By inserting a light rod into the handle, the possible length-effect of which we overcame by a weight on a heavy wire set into the rod and bent back under the handle in such a way that the center of mass was at the point of the forefinger, we secured better results. But the rod was presently dispensed with. We suspended the front weight, which had been rigidly attached to the front of the handle by a short wooden piece, on a cord and so eliminated the rotation-effect due to the swing of the weight. After lengthening this cord which held the front weight, so that its pendular period was about the same as that of a normal rod which would give the same 'kick-pressure' ratio as the two pressures from the handle, we took out the rod; a bare handle remained.

The front weight was 250 gr.; the back pull was 140 gr., increased and decreased by 20 gr. increments. To be sure, the two pressures were not wholly independent of each other; if the pull or 'kick' did not come directly under the point of contact between the hand and handle, any increase or decrease of the pull would be reflected by a change in the amount of pressure necessary to counteract the weight in front. We can not be absolutely sure that with *O*'s whose hands differed considerably in size we met this objection. But even if the front pressure varied somewhat from the amount of the weight, it could have done nothing more than change the ratio of 'kick' to 'pressure;' it would have been a constant error for any one *O*. According as it increased or decreased the absolute intensity, it would have made the small changes less or more easily perceived.

Two features connected with the lifting of the handle called for special practice; *O* must lift the handle in such a way that the two pressures came on simultaneously, and must oppose the back pull sufficiently to prevent that end of the handle from moving upward.

Fig. 18 gives the graphical representation of the results; the abscissae correspond with the five settings of the back pull which alone varied. Here are comparative length-judgments made under the same instructions as the judgments with actual rods. The results speak for themselves. There can be no doubt of the actuality of the perception, or of the effectiveness of small changes in the intensity-ratio of 'kick' to 'pressure' for altering it: the perception of length can be synthetically produced, and is so accurately conditioned that very small changes in a single pressure bring about corresponding changes in the meaning of length. A comparison of the results of series F IV with those of F V and VI (Figs. 24, 25 and 26) shows that variation of a single pressure is less effective for determining the perception of length than is a variation in both

pressures, changes in center of mass being the same; and a comparison of F V and F VI shows that, when the fore pressure is constant (see Table II), the back pressure variable, and the center of mass still the same for corresponding rods in the two sets, we get higher percentages of the 'longer' and 'shorter' judgments than when the reverse is true. We had not the results of section F before us when we performed the synthetic work.

The reports from the synthetic series show that the same two pressures demand attention under the synthetic situation as in the case of true rods.

Synthetic Series: (3—5+) "Strain-sensations run down wrist and are fused with the hand-sensations. The pressures come on forefinger and at base of thumb. These latter are objective" (Ca). (3—5+) "Pressure on index-finger most intense. More intense in second. Pressure on other fingers less intense. Pressure on thumb was uniform" (Ca). (5—3—) "In first, pressure on forefinger. Irregularity in change of intensity of pressure. In second, pressure was constant in intensity. Pressure on thumb and ball of thumb. Constant sensations in other fingers" (Co). (3—5+). "Pressure-sensations on forefinger and thumb, more extensive on thumb with second. Pressure on forefinger slightly preceded that on thumb. The two must be combined to give the total experience" (Co). (1—3+) "Pressure on forefinger and in palm of hand, at base of thumb and at tip of little finger. More intense pressure on palm of hand and little finger in second" (Co). (3—1—) "Very intense pressure at base of first finger and intense pressure at base of thumb. The pressure in the second case less intense. They came in simultaneously" (S). (3—5+) "Pressure deep at base of first finger and pressure on thumb, about the middle. Clearer and more intense on finger. Area shifted outward a little. In second, deep pressure on finger, more intense and changed in area, more spread out than first. Pressure on thumb less clear, more intense than in first" (S). (3—2—) "Dull pressure on forefinger and at base of thumb in both. Vague sensations in lower part of arm. Less intense in second case. The pressure at base of thumb less in second, gave meaning of slightly less unbalance" (H). (3—4+) "With the second, the pressure at the base of the thumb was more nearly equal in intensity to that on the forefinger, than in the first. No qualitative difference noted" (H). (5—3—) "Marked pressure sensations on base of thumb, in the first. Less clear sensations on forefinger. With second, the sensations on forefinger clearest. Those at base of thumb less intense" (Z). (3—1—) "First experience carefully noted. No definite idea of length. Judgment part of second. When analyzed, it was less intense at base of thumb and less strain in hand" (L).

V. Secondary Factors

Visual Imagery

All *O*'s reported visual images, more especially during the early series; Z reported images of this kind, more or less, throughout the whole of the two years. In all cases, however, the image was an immediately accruing process which derived its meaning from the

sensory complex in the hand. Frequently it was instable, fluctuating with changes of eye-convergence. Frequently *O* volunteered the statement that it came after the judgment and served merely a confirmatory purpose; it was his justification. Furthermore, the coming in of visual images bears a close relation to the giving, under the comparative determination, of judgments of absolute length and of reflective judgments. In fact, visual imagery was never more than a secondary factor in the perception; it never came alone or even first in temporal sequence; it was far more likely than not to occur in the case of the reflective judgment or to follow the judgment; and it never appeared when the judgment was a part of the perception.

The following reports show the symbolic character of the images, the temporal order of their occurrence, and the relation they hold to the notion of length and to the comparative judgment.

"Image and eye-movement. No notion of length. Just rod lying out there going into third dimension. This image carried right over into the second experience. But it had nothing to do with the judgment. Judgment made on basis of pressure solely" (G). "As soon as judgment was rendered, a visual image meaning length came in fragmentarily. Also eye movement. All as I am reporting and after." (G). "After judgment, visual gray spot for a second. Some eye-movements entered into spacing. Confirming" (G). "Process, gray thing, developed by eye-movement. When sighting where I thought the thing would be, image came in" (G). "Visual image. Eye-strain determined distance" (G). "Accompanying judgment was a composite visual schema of two lines, one longer than the other, in a vague way connected with the hand. Visual image not at all connected with the judgment" (L). "Slight visual imagery, very schematic. Not at all a reproduction. Visual image supplementary" (L). "Visualization not important. Weak, hazy, no clear outlines, insignificant" (F). "Visualization. Second, lighter gray. Spatially related, eight inches between them. Darker one farther out. Second lighter, nearer in" (F). "Visual image of a flashing up of the end of the rod gives a rough outline of length" (N). "Scrappy bit of visual imagery came in after the judgment. It meant the end of the rod" (H). "When I had lifted the last rod, I had a visual image of hand lifting two rods in succession. At first the two were not definitely outlined as to length. In a moment the image of the first rod appeared as extended about two inches beyond that of the second. This image was vague except at the hand" (Z). "In this experience I had very clear visual images. Clearest part of image connected with lifting the last rod was at distant end of rod. End of rod was much heavier than first. It seemed as if it were bent down at the end" (Z). "As soon as I lifted the first rod there appeared a definite extent in space from me. The nature of this I do not know, for almost simultaneously I had a visual image of a rod the distant end of which was the clearest part. I rather think there was something there before the visual image appeared" (Z).

The introspections show a relation, more or less definite, between the quality of the image and the perceived weight or length. For F the darker gray always stood for the rod producing the more intense pressure; Z distinguished the dark, solid-grained oak from the soft, light colored poplar; and H occasionally correlated 'light and short' with bright tint. Always, however the perception of weight or of length was primary. There is also a relation between the tendencies to

visualize and to objectify the stimulus-object. So long as *O* perceived length as bare length, unrelated to an object, the visual image did not enter; when he perceived length as the length of a particular object, the objectification often took place in visual terms.

Muscular, Strain and Articular Sensations

James[12] says of projected movements that all the intervening space must be represented in consciousness; that when the projection is to the end of a walking stick, the extent of this stick must be represented in visual, tactual or kinaesthetic terms. James is concerned with projection and not with bare length; but in the case of judgments of absolute length, with objectification of the experience, we have a very similar condition. We found muscular, tendinous and articular experiences reported by our *O*'s, for the most part as sensory and not as imaginal. Z reported them more consistently and for a longer time than any other *O*; Ca reported them during the weight-series. These sensations compelled attention by their intensive changes. The arm seemed to be the seat of the only variable experience; and hence to afford the psychologically adequate basis for making differential judgments. It may be that Z had recourse to visual and motor imagery and to the reflective judgment more than any other *O* because for him the successive perceptions were differently determined. That is, the clear and, for him, the significant part of the first experience might be arm sensations, for the second, the hand-complex. Then the visual images evoked by the two would come together as the common element for making the comparison. One of his reports will make this state of things clear.

(3—1—) "With the first rod, I had, first, sensations in the hand. I did not get a definite notion of length from the sensations in the hand. Then there were very marked sensations from the arm. Meaning of length took on more definiteness. These sensations from the fore-arm remained clear throughout the experience. With the second rod, I had sensations in hand and also given at the same time the notion of 'lighter.' Visual image of picture given by Ebbinghaus in connection with a discussion of looking at two grays. Notion of 'lighter' pretty well marked. Verbal-motor: 'Is it shorter?' Notion of length seemed to vacillate. Again sensations in arm came in, and seemed to help in the attainment of definiteness of length. Visual image of the two rods of about the same length. Heavier one dark and of heavy wood; the light one, a poplar rod. Judgment based on the visual images" (Z).

Experiences of such elaborate pattern were rarely reported; the imagery was more often of the motor or kinaesthetic than of the visual type. But almost without fail, when the successive perceptions had a different sensory determination, *i. e.*, when different sensory processes were clear, some type of imagery served to represent at least one member of the two experiences which came in for reflective comparison. It is significant that the other *O*'s seldom reported kinaesthetic sensations, but they almost never gave the dual determination of the two experiences, and Z rarely reported kinaesthesis in both experiences. Though, when sensations from the arm were clear, the perception of weight almost always intervened, it has yet to be shown that sensation from this source can function in any other than a

[12] W. James, *The Principles of Psychology*, 1890, II, 196.

vicarious manner; it is more probable that the strain and muscular sensations from the hand and, to a lesser degree, those from the wrist are the psychological correlate of the meaning of length; the part they play would still be secondary and not primary.

When we used the wooden 'spoon' to take up the 'kick' of the rod, L reported a 'thinness' or 'emptiness' of the experience, which he attributed to the elimination of the hand and wrist kinaesthesis; this kinaesthesis had served as a background for the total experience. With the heavier rods, S repeatedly reports muscular and strain sensations from the hand aroused by the leverage of the rod held in the horizontal position. All these things are very different, however, from the kinaesthetic image an *O* would derive from having laid the rod along the body and leg, or from having run the hand along its extent.

The Temporal Factor

There were at least three, more or less distinct, ways in which the temporal factor entered into the complex of experience to affect the judgments both of length and of comparative length. These were the quickness with which a short or light rod came up, especially when lifted after a longer or a heavier rod; the time, independent of the lifting, within which the pressure-pattern formed in the hand; and the degree of steadiness exhibited by the pressure, as 'dead' and unchanging or as 'live' and fluctuating. When the first of these modes was an element within the complex of experience, the judgment 'shorter' or a tendency toward that judgment followed, if the rod came up quickly, and contrariwise if it came up slowly. The former happened much more frequently than the latter. If *O* postponed judgment until after the rod became stable, he frequently reversed his judgment, if it was a matter of difference of weight.

Some of the pressure-patterns, in accordance with the second principle given above, formed immediately when the lifted rod became an effective stimulus, and remained unchanged throughout the experience; others showed a well marked course of development in intensity, extent, or (less commonly) quality. In general, an increase of intensity or extent carried with it the meaning 'longer,' as did a shift of quality in the direction of ache or strain.

The 'liveness' of the pressure, required by the third principle, differed from the change mentioned under the second heading in that it was not a growth in a constant direction. As a rule, an unusually 'dead' static pressure carried the meaning 'long;' the 'live' fluctuating pressure, the meaning 'short.'

These three aspects of the temporal factor hardly ever occurred in combination in a single experience. Illustrative reports follow.

"As pressure creeps it becomes more intense on index finger and less intense on little finger. Second pressure intermittent, up and down, on forefinger" (Ca). "Second gave very slow intermittent pressures" (Ca). "Sensations on forefinger varied in intensity, also in a temporal way" (Co). "The difference is largely a temporal one in the forefinger. In the first, there was fluctuation; in second, constant. Especially fluctuations in muscular and tendinous sensations of the forefinger" (Co). "Resistance less. Came right up" (N). "Longer ones more springy; shorter ones come right up quick without any springiness. Greater pressure on the base of the hand with the longer rods is there immediately upon picking them up.

As soon as they are raised the distribution of pressure equalizes and remains constant" (N). "Second pressure at base of hand variable in time. It had a 'lively' quality, go and come" (H). "A sudden intensity means weight; length comes slower" (F). "Strain-sensation in fingers and palm of hand. Also deep pressure, mixed together, and it grew in intensity relatively slowly. This gave the meaning 'longer'" (S). "Second, slower strain-sensation in fingers and on thumb. Rose to maximal intensity more slowly than first and carried meaning 'longer'" (S). "The temporal course of processes seemed to be the most important. Torque and temporal bound up very closely" (L). "Second experience, whole experience less in intensity and showed none of the temporal course. Second, 'shorter'" (L). "Temporal sequence in this case clearly observed. Slower in second case; *i. e.*, the one to which 'longer' attached. The thumb-complex rose slowly to full intensity" (L). "Arm went up so much faster than it ought. Too much energy. It was surprisingly short" (C). "First, pressure 'alive,' 'gay.' Perception bound up with greater intensity that meant 'dead' weight and not fluctuation" (G). "Second, got more intense and spread out as if rod was bending down" (G). "I thought both long because of temporal thing, gradual increase in intensity of pressure as they were picked up. The proportional relation of amount of intensity and the rate of increase were noted" (G). "There seemed to be something like a resistance, a give or vibration or something, that gave the notion of length as distinct from the dead pressure from heaviness" (G). "Second seemed to go up. Sudden change in pressure, lightening of pressure, gave notion of 'short'" (G). "Fluctuation, more and then less intense, gave perception of fairly long dipping rod" (G).

Pressure Gradient

There are, no doubt, other secondary criteria upon which the perception of length, in the sense of this study, may be based. We have found, *e. g.*, that if a ring is made fast to the skin by means of a gummed tape, and a pull is exerted on this ring in a direction away from the body while at the same time a pressure of slightly different intensity operates upon a nearby spot on the skin, the perception of length follows: the extent is in the direction of the 'pressure' if this is more intense than the 'pull;' in the opposite direction, if it is less. Gradients in either direction are thus adequate to evoke the meaning of length, provided always that they are opposite and slightly different in amount. Moreover, the steepness and direction of the gradient, in the hand at least, are factors that on occasion play a part in determining the meaning of length.

The Absolute Length-Judgment

As in the case of lifted weights, the judgment of absolute length appeared; all *O*'s reported it at times. The judgment usually occurred with rods of the one extreme or the other; practically the second one was judged before it was lifted. The general instructions failed to the extent that a special disposition or attitude might develop which prevented *O* from doing his assigned task. Suppose, *e. g.*, that *O* judges the first rod as 'very long' in absolute terms; then he will expect, pre-judge, the next as 'shorter:' the consequent 'set' for the next rod is 'short;' the characteristic intensive, temporal and other differences are anticipated.

This absolute judgment is, however, of little concern to us in our analysis of the non-visual perception of length, since there is no indication that its determination is in any way different from that of the perception of bare length or of the perception of difference of length. We note only that the visual image, more frequently than any other process, carries the meaning of absolute length. Visual image, absolute length and the reflective judgment are bound up together. The two absolute meanings, visually determined, must be brought into a logical relationship before a comparison is possible. The situation is somewhat different in the case of judging the length of a single rod. Illustrative reports are:

"Visualized two points in space. First, far out; second, near. Two absolute judgments. Relative judgment an inference" (F). "I got the meaning of absolute length with the first. This judgment of absolute 'shortness' for the first gave me a 'set' for 'longer.' I expected the second to be longer. I can't say whether that imposed judgment or not" (H). "Absolute judgment of first as 'long.' Set to expect 'shorter'" (G). "Absolute judgment of first 'long.' Set off by pressure-pattern" (G).

VI. On the Computation of a Limen

The method of Constant Stimulus Differences may and usually does give a *DL*. Still following Urban we sought to use our data for this purpose. We computed the *h′* and *L′* for the results of the several *O*'s in the early series; but there we were halted. What is the unit of stimulus in terms of which a *DL* might be expressed? We had three sets of variables, all of which varied together in direct proportion. Which should be used in correlation with the psychological experience? At first we thought that the *DL* should be expressed in terms of weight; for applied weight is the mechanical stimulus which produces sensations of pressure, and resisted weight, muscular and strain sensations. But the results shown in Fig. 2, as well as those of the other series in which the weights were the same for all rods, prove that the *DL* cannot be found from the objective differences in the weight of the rods. So the graphs given in Figs. 4 and 17 show flat and irregular distributions; the 'kick' and 'pressure' were not equal or constant, but their ratio did not change. Weight is out of the question.

Before the work was far advanced we had decided that length, in and of itself, could in no way affect differentially the arousal of sensory experience; hence we could not correlate length with judgments of length. The results (Figs. 5 and 15) prove the justice of this conclusion. Nor are weight and length combined as co-variables adequate so to determine the perception that accurate comparative judgments are possible (see Figs. 4 and 14). Judgments of length were possible,

since the adequate stimulus for the perception of length was present, but they were undifferentiated. Length, too, is out of the question.

Can we, then, state a *DL* in terms of center of mass ? We have already remarked that the intensity-ratio of 'kick' to 'pressure' varies with every change in center of mass; and we have said that it is precisely this ratio that determines, psychophysically, the non-visual perception of length. The distance, in absolute terms, from center of mass to fulcrum-point at the forefinger varies directly with the intensity-ratio of 'kick' to 'pressure,' and the relation of this ratio to another of the same kind in two successive experiences determines the perception of comparative length. Have we here a moment in terms of which we may express a correlation between experience and stimulus-object ?

In the first place, we note that changes in this intensity-ratio are effective for modifying the perception only within certain limits, the lower of which we have fairly established. The *O*'s failed to perceive length when the intensity of the 'pressure' became twice that of the 'kick' (series A III, B IIIa and F I). The upper limit we did not determine. In the second place, a number of other secondary factors enter in to influence the perception of length. We have already discussed at length the relation of absolute intensity, as a correlate of weight, to the perception. In addition there are the elements of time, the character of the pressure-gradient, and the general sensory background from the muscles, joints and tendons of hand, wrist and arm, all of which not only touch off the meaning of relative length but may actually modify the perception.

The results from the 'spoon'-series prove, nevertheless, that there can be no perception of length without the two pressures. Here the perception appeared only if there were movement of the rod; as the rod swayed or vibrated there was generated a duality of impression within the limited area of the single contact-point. There is one instance in which the two sensations seem at first sight to come in succession rather than simultaneously: the case of the rod balanced on the end of the finger. As the rod begins to tip over, there comes the knowledge that it is an extended object and not a compact mass. The pressures are, however, not really successive; pressure becomes intensified in one part and weakened in the opposite part. If the end of the rod be pressed obliquely against the skin there is no perception of length. It is only as movement begins, causing the inverse change of intensity in opposite parts of the stimulated area, that we perceive length.

Thus the primacy of the two related pressures forces itself upon us once more.

We thus arrive at the conclusion that the primary item of experience, the underlying and essential condition for the perception, is a ratio between two opposing pressures held together by certain directional and temporal relations. But *for this ratio there is no directly corresponding element or moment in the stimulus-object.* Center of mass, it is true, bears a constant direct relation to this ratio; only, if we have recourse to it, the physical terms in which we express the limen will be two steps removed from the essential item of direct experience. The primary psychological condition of the perception transcends the stimulus-object.

VII. On the Application of Weber's Law

As regards Weber's law, there is a characteristic of our curves of distribution which may have an indicative value. In a majority of cases, when we consider those series only which gave X's, the curves for the judgment 'longer' do not rise as high as the curves for 'shorter,' and most of the inversions and plateaus lie on the upper side of the standard. Our steps, whatever their nature, represent (except in section F) an arithmetical and not a geometrical progression. Stimuli which vary in this way should, under a strict working of Weber's law, give psychophysical results such as we obtained. Our interpretation of the perception of length as based upon a ratio of intensities is thus, so far, confirmed.

Conclusions

Our analysis shows that the non-visual perception of the length of a lifted rod is based primarily upon the experience of two opposed pressures whose intensive ratio falls within certain limits.

There are at least four factors which join with the two opposed pressures, singly or in combination, to complete the perception. These are absolute intensity of the impressions, their temporal course, the pressure gradient formed, and the muscle and strain sensations from hand and arm.

Visual and kinaesthetic images serve to establish the length in absolute terms. They may serve the end of comparison, but then imply a reflective judgment.

The perception is given as accurately in the passive as in the active state.

The psychological substrate of the meaning of length has no direct physical correlate in the stimulus-object.

The perception can be synthetically produced by the application, under certain conditions, of two opposed pressures.

A NEW FORM OF STIMULI FOR LIFTED WEIGHT EXPERIMENTS

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The questions regarding the form and material of the stimuli to be used in lifted weight experiments have been variously answered by different experiments. Fechner [1] placed his weights in holders which were grasped and lifted with the palm of the hand upward. Galton [2] used simple shotgun shells loaded to the proper intensity with successive layers of shot, cotton wool and wads. These weights were grasped by the subject "between his finger and thumb, the finger pressing against the top, the thumb against the bottom of the cartridge." There is a set of brass weights loaded with paraffin made in this form by the Cambridge Scientific Instrument Company, Ltd. Sanford [3] suggested the use of envelopes weighted with sheet lead, to be lifted vertically suspended between the thumb and forefinger. Presumably weights of this type were intended only for use in an elementary laboratory course. Fullerton and Cattell [4] employed flat wooden boxes weighted with shot and cotton, which were lightly grasped on the side with the thumb and fingers. Urban [5] used weights of the same general form made of hollow brass cylinders loaded with shot and paraffin, which were also lightly grasped on the side between the thumb and fingers. Inasmuch as paraffin is not entirely anhygroscopic, solder was substituted in a second set of weights exactly similar in form and size to those used by Urban.[6] A set of weights made of hard rubber and weighted with shot, advertised in 1894 by the Garden City Model Works, is

[1] G. T. Fechner. *Elemente der Psychophysik.* 2nd Ed., 1889, I., 97f.
[2] F. Galton. *Inquiries into Human Faculty and Its Development.* N. Y., 1883, 34ff, 370ff.
[3] E. C. Sanford. *A Course in Experimental Psychology.* Boston, 1898, 413.
[4] G. S. Fullerton & J. McK. Cattell. *On the Perception of Small Differences.* Phila., 1892, 118f.
[5] F. M. Urban. *The Application of Statistical Methods to the Problems of Psychophysics,* Phila., 1908 1ff.
[6] S. W. Fernberger. On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli. *Psychol. Rev.,* XXI., 1914, 337f.

mentioned by Titchener.[7] They consisted of 9 cylinders, 1½ inches in diameter and 4 inches high.

It seems curious that experimenters have overlooked the obvious fact that the size and form of the stimuli employed in this sort of experimentation may have an effect upon the measures of sensitivity. Experimenters have directly compared the results for lifting weights by the Fechner handles with the results for lifting by grasping the weight itself. It seems obvious that the manner of lifting a weight must affect the results. The processes underlying the formation of judgments in lifted weight experiments are exceedingly complex and involve pressure, muscular, articular and tendinous sensations with widely different points of origin,[8] which might be expected to differ with the various forms of weights and the manner of lifting them. In the face of ignorance the safe course is to use a single standard form. Weights of the size and form suggested by Urban have had a large and satisfactory usage in the hands of a number of recent investigators. They are comfortably grasped between the thumb and forefingers.

The materials of which the weights are composed also require standardization. In the first place the physical weight must be kept constant within the limits of error of the experiment. Urban [9] found that the wooden weights of Fullerton and Cattell varied, with differences in atmospheric temperature and moisture, sometimes more than 15 mg. The brass weights constructed by Urban and the writer overcome this difficulty. Such a set, loaded with solder, in use in the Clark Psychological Laboratory for the past eight years, has not shown for any single weight as much as 10 mg. variation.

Ide [10] has recently found that warm or cold stimuli are overestimated when compared with stimuli which do not arouse temperature sensations. Ide compared cold stimuli of 46° F and warm stimuli of 147° F. with neutral stimuli of 70° F. The resulting variations in the points of subjective equality for the method of constant stimuli are considerable; in some cases the difference is more than three grams.

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[7] E. B. Titchener. *Experimental Psychology*. N. Y., 1905, II., Part 2., 265.

[8] cf. S. W. Fernberger. An Introspective Analysis of the Process of Comparing. *Psychol. Mono.*, XXVI (Whole No. 117), 1919.

[9] Urban. *op. cit.*, 2f.

[10] A. L. Ide. *The Influence of Temperature on the Formation of Judgments in Lifted Weight Experiments.* Univ. of Pennsylvania Thesis, Phila., 1919, Pp. 25

Ide has investigated this problem systematically but the general observation appeared in the literature many years ago. In 1846, E. H. Weber [11] observed that a cold coin at —4° to 7° C. placed on the forehead seemed as heavy as two superinposed coins of the same size but of a temperature of 37° to 38° C. Some years later Szabadföldi [12] repeated this experiment but extended the range of warm temperatures beyond that employed by Weber. Szabadföldi found that both very cold and very warm stimuli are overestimated when compared with neutral stimuli which do not arouse temperature sensations. These results are entirely in accord with Ide's findings although in one case the experimenters were dealing with passive pressure and in the other case with active lifting.

In some recent experiments performed by the writer during the winter, the subjects sometimes complained that the brass weights felt cold. Metal readily gives rise to thermal sensations because of its high thermal conductivity. For this reason we have constructed a set of weights from hard rubber which has a low coefficient of conductivity, and which further recommends itself as anhygroscopic, durable and easily worked.

The coefficient of thermal conductivity (calories per cm. per sec. per degree) for yellow brass is about 0.20 and for vulcanite about 0.002.[13] In terms of stimulus and of sensation, if thermal conductivity were the only factor, this difference would mean that the range of temperatures which do not arouse temperature sensations would be 100 times as great for hard rubber as for brass.

Hard rubber seems to have been little used for lifted weights. We noted above one set of rubber weights. The Fechner weight holders listed by Spindler and Hoyer of Göttingen in their catalogue of 1910 have hard rubber handles. But neither of these forms of weights have been used in important experimental work. On the other hand, it has been almost universally recognized in aesthesiometric work that metal points arouse temperature as well as pressure sensations

[11] E. H. Weber. *Der Tastsinn und das Gemeingfühl.* 1846 and also Tastsinn in Wagner's *Handwörterbuch d. Physiol.,* Braunschweig, 1846, III., Part 2.

[12] M. Szabadföldi. Zur Physiologie des Tastsinns. *Unters. z. Naturlehre d. Menschen.* IX., 1865. For an account of these experiments *cf.* O. Funke. Der Tastsinn und die Gemeingefühl in Hermann's *Handbuch d. Physiol.,* Leipzig, 1879, III, Part 2, 320f. and also G. T. Ladd. *Elements of Physiological Psychology,* N. Y., 1887, 347f.

[13] *Cf.* A. Winkelmann. *Handbuch der Physik,* Leipzig, 1906, III, Part 1, 489 & 505.

and that they are therefore to be avoided. Titchener,[14] for example, remarks in discussing the Griesbach aesthesiometer, —"The points are of metal. This is a disadvantage as it introduces the temperature error."

It is difficult to discover in the literature the origin of the use of non-metallic materials to eliminate thermal sensations. Hall and Donaldson[15] in 1885, in discussing the influence of temperature in the perception of movement on the skin, say: —"It was soon found, however, that these limits overlapped, *i. e.* that an intermediate temperature of a metallic point could be so chosen that it would stimulate both heat and cold spots. We therefore had recourse to non-conducting cork points of such shape that the smooth and rounded edge of a right-angled triangle was applied to the skin transversely to the direction of motion. Thus the sensations of temperature appeared to be entirely eliminated." In 1887, Hall and Motora[16] quite casually speak of the use of hard rubber for the same purpose. In the manufacture of aesthesiometers, bone and ivory have also been employed as non-conducting materials to eliminate temperature sensations.[17]

The new set of weights in the Clark Laboratory conform to all the requirements which we have noted. They are hard rubber cylinders, 2½ inches in diameter and 1 inch high. Hard rubber can be purchased in round bars, just 2½ inches in diameter. The cylinders are cut off and hollowed out at one end to a depth of ½ inch; a wall at the sides ⅓ inch in thickness is left. A round brass plate, ⅛ inch in thickness, is inserted into this cavity and is held in position by two bolts and nuts which pass through holes bored in the top of the weight and in the brass plate. The heads of the bolts are counter-sunk into the top of the weight so that they are below the surface. A small number is stamped with a steel die on each weight for purposes of identification.

When the weights are calibrated, the brass plate is unscrewed and solder is melted on it until the rubber shell, the brass plate, the nuts, the bolts and the solder weigh just a little more than the desired amount. Some of the solder is then carefully scraped away until the desired weight is obtained.

[14] E. B. Titchener. *Experimental Psychology*. N. Y., 1901, I., Part 1., 382.

[15] G. S. Hall & H. H. Donaldson. Motor Sensations on the Skin. *Mind*. O. S. X., 1885, 568.

[16] G. S. Hall & Y. Motora. Dermal Sensitiveness to Gradual Pressure Changes. *Amer. Jour. of Psychol.*, I., 1887, 74.

[17] *Cf.* A. Gemelli. Un nuovo estesiometro. *Atti. d Soc. Ital. d. Sci. Nat.*, LII., 1913, 193-200.

This present set of weights has been tested a number of times during a period of two weeks in which there have been great variations in temperature and humidity. In no case did any weight vary as much as 5 mg., a variation which is far within the limits of accuracy required in the lifted weight experiment.

Under experimental conditions these weights actually do not arouse temperature sensations. Even when placed for several hours on a very hot radiator or outdoors in air at 20 F., several subjects found that the hard rubber weights were only slightly cool and warm, whereas the brass weights were almost painfully cold and painfully hot. Presumably even unusual changes of room-temperature with the hard rubber weights will not arouse sensations of warmth or cold at all.

THE PSYCHOLOGICAL EXAMINATION OF CONSCIENTIOUS OBJECTORS [1]

By Mark A. May, Syracuse University

Psychological examining in the United States Army yielded a large body of results that may be considered as by-products. Most of these by-products will not appear in the official report which is now in the course of publication. In order that these incidental results be made available it has been thought wise to publish them as separate studies. The present study is a report made by the writer to the office of the Surgeon General, December 1918. It is an attempt to present in a summary fashion the available information concerning the intelligence, education, grounds of objection, social and political history of conscientious objectors..

The first psychological examinations of conscientious objectors were made in compliance with a letter from The Adjutant General of the Army dated March 6th, 1918. At that time there were no psychological examiners on duty at most of the camps. These first examinations were made by the psychiatrists and medical officers or by Special Boards. The reports of these early examinations are very diverse in character and extremely difficult to summarize. On April 22, 1918, Major R. M. Yerkes summarized these data in a report for the Secretary of War. The essential facts of this summary are embodied in this report. As soon as psychological examiners were on duty at most of the camps and cantonments a letter was sent out from the Surgeon General's Office, Division of Psychology, directing how the examinations should be conducted and the reports should be made. In June, 1918, a special examination for conscientious objectors was sent out to the camps. A copy of this examination appears in an appendix to this paper.

The data presented here are taken from about 30 reports sent to the Surgeon General's Office by the various psychological examiners. Some camps made several reports, others made one and some made none. The reports available for this summary cover about 20 camps and represent about 1000

[1] Published with the approval of the War Department.

objectors. This is a little less than one half of the total number of conscientious objectors in the army. From a careful examination of the available information the writer is of the opinion that the 1000 cases presented here is a fair sampling of the total number.

The intelligence of conscientious objectors, as measured by the army mental tests, is on the average above that of the white draft of the army as a whole. By the army mental tests intelligence is rated on a scale of A, B, C+, C,C—, D, D—. The following table compares the intelligence ratings of the white draft of the army and the conscientious objectors.

		White Draft[2] (94,000 Cases)	Conscientious Objectors (1,000 Cases)
A	"Very Superior"	4.1%	8.7%
B	"Superior"	8.0%	15.2%
C+	"High Average"	15.2	22.6
C	"Average"	25.0	24.8
C—	"Low Average"	23.8	16.8
D	"Inferior"	17.0	8.7
D—	"Very Inferior"	7.1	3.1

These figures show that 46.5% of the conscientious objectors are above the grade of "C" which is *Average,* while in the army as a whole only 27.3% are above "C." And conversely 28.6% of the conscientious objectors are below "C" while 47.9% of the army as a whole are below "C." Another interesting fact about this table is that the ratio of A & B men of the army as a whole to the A & B men of the conscientious objectors is about 1:2. Conversely this ratio for D and D— men is about 2:1. There is absolutely nothing in these data to justify the hasty conclusion that objection to war is due to the low mentality of the objector. It may be true that some objectors can be classed as fanatics, others may be said to be stupid, but it can safely be said that at least 97% of the conscientious objectors reported here have sufficient intelligence to know what they are doing.

Educational data were available on 880 objectors. The following table shows the percentages of these men who reached the various grades in school.[3]

[2] These figures are quoted from page 1076 of the Report of the Surgeon General to the Secretary of War, 1919.

[3] It is regretted that the educational data of the army as a whole are not available at this time. They will appear in "Psychological Examining in the U. S. Army," Memoirs of The National Academy of Sciences, vol 15 (In press).

Grade Reached	Per Cent.	Grade Reached	Per Cent.
0	100	H. S. I	42
1	99	H. S. II	29
2	98	H. S. III	24
3	97	H. S. IV	18
4	94		
5	88	Col. I	12
6	80	Col. II	9
7	70	Col. III	6
8	58	Col. IV	3
		Professional	.5

It will be pointed out presently that about 50% of the men in this report are Mennonites. Less than 10% of the Mennonites went beyond the 8th grade. The 12% who reached college were either Socialists, Dunkards or Friends.

The religious denominations of 1060 conscientious objectors are as follows:

Mennonites (Old and New)	342
Mennonites (Defenseless)	13
Mennonites (Swiss)	12
Mennonites (Amish)	99
Mennonites (Old Amish)	88
	554
Friends (Quakers)	80
Brethren, Church of the	67
International Bible Students (Russellites)	60
Dunkards	37
Israelites of the House of David	39
The Church of Christ	31
The Church of God and Saints of Christ, Etc	20
Seventh Day Adventists	20
Methodists	14
Pentecostal Assembly	13
Catholics	9
Assembly of God	9
Missionary Church	8
Pentecostal Church of the Nazarene	8
Lutherans	8
Christians (Campbellites)	7
Christadelphians	7
Holiness	6
Apostolic Christian	6
Baptist	5
Brethren (Plymouth)	5
Church of the First Born	4
Congregationalists	4
Presbyterians	3
Zionists	3

Other denominations of less than 3 are: True Light Church,

Christian Science, Brethren in Christ, Baptists (Free Will) Evangelists, Baptist (Austrian), Baptist (United), Brethren (United), Christian Lights, Gospel Hall, Universalists, Stundists, Rosacrucians, Mazdazman, Brotherhood of Mystics, Ethical Culture Society. In addition to this list 55 cases are reported as having no denominational affiliations at all.

Attention has already been called to the fact that about one half of the conscientious objectors are Mennonites of one branch or another. The Friends, Brethren, Dunkards, International Bible Students (Russellites), and Israelites of the House of David constitute about 25% of the total. These taken with the Mennonites make up fully 75% of the conscientious objectors.

It is interesting to observe that all of these sects, with the possible exception of the Friends, believe in the literal interpretation of the Scriptures. As will be seen later their appeal is usually to the Bible and to their church creed. Moreover they are all more or less clannish. This is especially true of the Mennonites who live in colonies in the middle west. In some instances they constitute a large part of the population of one county.

The list of religions "organized prior to May, 1917 and well-recognized, whose creeds forbid participation in war" are relatively few. Such a list has been prepared by Mr. Hunt of the Census Bureau and is as follows:

Mennonites, 16 bodies (including the Amish);
Friends, (Quakers) 4 bodies;
Dunkards, three bodies;
Brethren, four bodies;
German Baptists; two bodies;
Churches of Christ;
Christadelphians;
United Zionists;
Assemblies of God;
Pentecostal Church of Nazarene;
Apostolic Holiness;
Fellowship of Reconciliation (not a religious sect);
International Bible Students Association (Not an organized sect).

The denominational lists have interest to the psychologist because in conducting an examination it was found necessary to know something of the religious background of the objector. (See the examination at the end of this article).

The grounds of objection to war are in general three, religious, social and political. The religious objector makes

his appeal to the Bible, church creed and to conscience. The social objector makes his appeal to individual freedom. The political objector usually bases his objection on the ground of alien citizenship. Out of 958 cases, 90% object on religious grounds; 5% on social grounds; 3% on political grounds and 2%on ethical grounds. It is quite obvious that the problem of the conscientious objector is a problem of dealing with religions.

In many cases records were kept of the specific objections which were made. Such records were available of about 500 cases. Object Because:

War is forbidden by church and creed	115
War is forbidden in the Scriptures in general	125
War is forbidden by Christ	95
War is forbidden by the commandment	60
War is forbidden by conscience	120
War promotes evil	16
War is wrong in and of itself	21
War is capitalistic and Imperialistic	10
War is getting autocracy	7
Have friends among the enemy	7
Not citizens of this country	6

Other objections not occurring very often but which are rather interesting are:

Objector believes that he will lose his soul and go to Hell if he should disobey his church and his conscience and go to war.

Objector says he cannot obey God and Mammon. Thus he could not obey God and the army sergeant or captain at the same time.

Objector refuses to use carnal weapons. Prefers the sword of the spirit.

Objector refuses to participate in war because it involves hatred toward the enemy.

Objector believes that war is punishment of the unjust and the just are, therefore, exempt.

Objector fears that the temptations of army life will lead him astray.

Objector refuses to be " yoked with unbelievers" and thereby jeopardize his chances of salvation.

Objector believes that war is legalized murder.

Objector refuses to use physical force to the extent of separating soul from body.

Objector promised parents and pastor that he would not fight and feels that he must remain true to his promise.

Objector says " war makes the rich richer and the poor poorer."

Objector doubts America's motives.

Objector says he was not allowed to vote on the war.

Objector says that by staying out he will encourage socialism in Germany.

The political affiliations of about 340 objectors were secured. Of these 340 there were 45 Republicans, 62 Democrats, 22 Socialists, 26 Prohibitionists, 5 Independents, 2 Progressives, 3 not citizens, 3 other parties, and 75 no political affiliation at all. It is interesting that about one half this list have no political affiliations. Nearly all of these non-voters are either Mennonites, Dunkards, Brethren, or I. B. S. A., or Friends. But this is not as significant as it seems to be since it has been pointed out that about 75% of all the objectors belong to these denominations. The writer is of the opinion that there is no causal connection between this apparent disinterest in political affairs and objectors to war. It should be remembered that some religious denominations actually forbid the communicants to take any part in politics on the grounds that they must keep " unspotted from the world."

As far as the information goes very few objectors belong to fraternal organizations. The reports show that more than 90% have no fraternal affiliations. The ten per cent that do belong are scattered over a large number of organizations. Speaking generally one might say that the average conscientious objector is a non-voter, belongs to no social organizations, is not interested in community enterprises. The writer has examined more than fifty objectors and has not found a single individual who had even been superintendent of a Sunday school or held a responsible position in the community.

At the time of the psychological examination the examiner in some instances asked the objector what kind of service he would be willing to render. Reports in 718 cases show the following choices:

Willing to accept regular military service	16
Willing to accept non-combatant service	160
Willing to accept farm furlough	275
Unwilling to accept any kind of service that is any way connected with the military machine	267
	718

The Board of Inquiry with whom the final disposition of these men rested did not in all cases give the objector the kind of service he wanted. It did, however, consider the objector's preferences. The following table is quoted from W. G. Kellogg's book, The Conscientious Objector, page 127. It shows the final disposition made of 2100 cases.

Recommended for farm or industrial furlough......	1500
Recommended for Friends' Reconstruction Unit....	88
Recommended for noncombatant service...........	390
Assigned to general military service (insincere)....	122
	2100

The factors determining the kind of service an objector was willing to accept are hard to find. They appear to be neither religious nor intellectual. There is little difference in the average intelligence ratings of the men choosing these various forms of service.

Many psychological examiners made an effort to get at the objector's occupational history. In many cases these data would throw some light on the case. The occupations of 888 objectors were obtained. The more frequent ones are listed here.

Farmers	548	Printers	11
Clerks	40	Salesmen	11
Laborers	27	Miners	10
Machinists	20	Chauffers	10
Students	24	Bookkeepers	6
Teachers	22	Engineers	5
Carpenters	17	Factory Workers	5
Ministers	16	Electricians	4

A total of 90 different occupations is represented by these 888 objectors. They run the whole gamut from accountants to zinc workers including one saloon-keeper and a gunsmith. The most striking fact about this list of occupations is the large number of farmers. This in turn is due to the preponderance of Mennonites of whom 90% are farmers.

The available data on the birth and ancestry of conscientious objectors show that out of 720 cases 90% are native born. No accurate information is available on the ancestry of objectors but what is at hand seems to indicate that approximately one third were of American parentage, one third German, and one

third English, Scotch, Russian and Scandinavian. The writer is extremely doubtful if parentage or birthplace has anything to do with an objector's refusal to take part in war. The large percentage of German parentage can easily be accounted for by the fact that most Mennonites are of German descent. Conscientious Objectors cannot be intelligently disposed of by simply calling them pro-German.

The degree of sincerity of a conscientious objector is a thing almost impossible to determine. It is difficult enough to find a test that will identify him, much less to find a measure of his sincerity. Of course, it was not the business of the psychologist to pass final judgment on the sincerity of an objector. But since the psychologist was often called upon to testify in courts-martial proceedings, to pass on an opinion to company commanders, and to furnish testimony to the Board of Inquiry it was necessary that he be able to formulate some opinion as to the sincerity of the objector. One of the chief purposes of the examination herewith attached was to aid the examiner in making up his mind. This examination seeks (1) to determine the objector's intelligence and to find out if he has any mental abnormalities. (2) His educational and occupational history is gone into with a view to getting at his interests. (3) Questions are asked about his church creed and his religious experiences to see how well informed he is. The assumption is that if an individual bases his objection to war on a church creed he ought to know something about it. (4) His moral habits and his social outlook are reviewed. In short the psychologist attempts to get as many lines as possible on the objector's character and social trustworthiness. All this evidence is put together when the psychologist makes his report. Such questions as "What would you do if a German should attack your Grandmother?" are carefully avoided. Questions of this sort are not tests of sincerity and show poor judgment on the part of the examiner. The non-resistance issue was not raised. It leads nowhere.

The Board of Inquiry passed on approximately 2100 cases.

The disposition of these cases was as follows:

Found to be sincere objectors and recommended for farm or industrial furloughs	1500
Found to be sincere objectors to combatant but insincere as to noncombatant service	219
Found to be sincere objectors of combatant but willing to accept noncombatant service	156

Found to be sincere objectors to combatant service but willing to work in reconstruction hospitals.........	15
Found to be insincere both as to combatant and noncombatant service	122

(This table is quoted from Mr. Kellogg's book cited above).

Unfortunately no reports are available showing how well the psychologists' recommendations correlated with the findings of the Board. It was the writer's experience in examining objectors that after carefully applying the examination little doubt remained in the mind of the examiner as to the objector's sincerity or insincerity.

Some general conclusions regarding the psychology of conscientious objectors may now be given. In the first place conscientious objection to war cannot be attributed to inferior intelligence. While it may be true that some religious sects are below the average intelligence of the army, yet they are not far enough below it to have any effect on objection to war. Taken as a whole the conscientious objectors are far superior in intelligence to the white draft of the army taken as a whole. Neither can objection to war be laid at the door of abnormal mental states. It is true that some objectors exhibit mental instability, possibly some are religious fanatics, yet a great majority of them are sane. Moreover conscientious objection to war is not a special case of the "blind leading the blind." Many objectors adhere rigidly to their creeds and follow the instructions of their pastors and so on, but many of them are keen thinkers. They are quick in debate and logical in their arguments. Mental questioning rather than unquestioning is more often the cause of objection.

From a survey of the available data three types of conscientious objectors stand out rather clearly. First there is what may be called the *religious-literalist* type. To this class belong most of the Mennonites, Dunkards, some branches of the Brethren and many of the obscure denominations mentioned above. If an objector of this type were picked at random he might be characterized as follows: He is a Mennonite, born on a farm in a middle Western community. His parents together with others of their faith settled there many generations ago. He went to the country church school. There he was taught the Bible and some simple rules of living. He quit school at the 8th grade and went to work on the farm. He attended church more or less regularly. He was taught to respect his minister and his views of life. He perhaps read

the county newspaper and the Bible, outside of this he has done little reading. His information is limited to the happenings of his own community. He knows that we are at war with Germany but has no idea why. He has no social vision, no national consciousness. So when creed, minister, parents and friends tell him that war is wrong and that he must not fight, what could be expected of him ? For a man like this not to be a conscientious objector would violate all the laws of heredity and environment that operate to make men pursue certain courses of action. This type of man constitutes fully 75% of the conscientious objectors.

Secondly, there is the *religious-idealist* type. His appeal is not to church or creed, although he is usually a church member. He appeals directly to conscience and to the teachings of Jesus. He was probably born in a city or an enlightened community and is probably a high school or college graduate. Some men of this type hold advanced degrees from large universities. He is a true Christian and has for years tried to follow closely the example and teaching of Jesus. He is very intelligent, in fact, he may measure up with many officers on the army intelligence scales. He is a thoroughgoing idealist and is entirely unwilling to sacrifice his ideals by resorting to expediency. He perhaps does not have the support of his pastor or friends. He has but one principle of life and that is the principle of love. He believes in applying this principle even in dealing with his enemies. In most cases he refuses to make any compromises and for this reason he is the type usually found in the Disciplinary Barracks. The fault with his type is not that he does not have social vision, but that he has too much.

Thirdly, there is the *socialist* type. He is likely to be well educated and very intelligent. He is well informed on the vital social and economic problems of the day. His patriotism knows no national limits. Here again it is not that he is not patriotic, but he is unusually so. He believes in the Brotherhood of Man, a United States of the World, a League of Free Nations, an ideal state of society in which all shall have equal rights. He is not willing to fight the proletariat of Germany just to get at the ruling classes. He may even believe in the use of force and in fighting, but it is capitalism and imperialism that he wants to fight. The problem with him is that he entirely fails to appreciate "national" problems. The last two classes of objectors constitute about 25% of the whole.

APPENDIX

EXAMINATION OF CONSCIENTIOUS OBJECTORS

This examination outline is supplied to every examining staff in order that it may be used as seems desirable. Its use is not ordered but it is believed that it will greatly assist examiners who lack experience.

Purpose:

1. To determine the degree of intelligence of the conscientious objector, with a view to finding out whether his objections are due to any abnormal mental conditions.
2. To obtain the *specific grounds* of the conscientious objector's objections, and to see how well grounded he is in them (not how consistent).
3. To ascertain the extent of the conscientious objector's information concerning the history and tenets of his faith, and of the organization (if any) to which he belongs. This is for the purpose of ascertaining his objections in somewhat the same way as that employed by Personnel Officers with reference to skill in occupations.
4. To determine the degree of the conscientious objector's objections; just what he is and is not willing to do in the war; e. g., wear the uniform, take the military oath, enter non-combatant service or non-military service, reconstruction, etc.
5. To determine the degree of his sincerity.

Note:—The following examination is worked out with a view to covering as many kinds of cases as are likely to occur. It is not necessary that the entire examination be given to any one man. As soon as the examiner has reached the point where he can be reasonably sure that he has sufficient information to satisfy the above requirements, the examination should cease.

I. *Mental Age Examination.*

If the subject speaks English, use Stanford-Binet or Point Scale; if illiterate or non-English speaking, use Performance Test. In any case give as many tests involving verbal responses as possible. Write down all responses in full. Watch carefully for all signs of malingering, for responses that indicate a psychopathic condition, and for responses that have a definite religious trend. Calculate the mental age at once, and let your subsequent questions be governed by the degree of the subject's intelligence.

II. *Personal and Family History.*

1. Are your parents living ? If not, state cause of death.
2. Any mental disease or peculiarities existing in family ?
3. What blood relationship exists between parents ?
4. Have you ever been troubled with sleeplessness, excessive worry, discontent, or any nervous disability?
5. Have you ever had any serious disease or physical defects?

III. *Educational History.*

1. Where did you go to school ? How long?
2. How old were you when you quit ? Why did you quit ?
3. What subject interested you most ?
4. How did you get along in school ?
5. What books and magazines do you read most now ?

IV. *Occupational History.* (Important, if subject is Socialist.)

1. What was your occupation just before joining the Army? How long did you work at it?
2. How many different jobs have you had in your life? Where? Why changes?
3. Do you belong to a trade union? Why or why not?
4. Did you ever participate in a strike?
5. Did you earn enough money to live comfortably?

V. *Religious History and Practices.*

1. What is your denomination? What branch? How long have you been a member? Do you have certificate of membership?
2. To how many different denominations have you belonged? Why changes?
3. When, where and how converted? How many times? (Note especially specific religious experiences, such as visions, divine messages, etc.)
4. What is religion of father, mother, brothers and sisters?
5. Have you ever studied for the ministry? Every preached? How long? Where?
6. Do you read the Bible daily? How much? What parts interest you most?
7. What religious books other than the Bible do you read?
8. How often do you go to church?
9. How often do you pray? For whom and for what? Do you pray for the Kaiser?
10. Have you ever got direct answers to prayer?
11. Did you attempt to convert others to your religion?

VI. *Moral History and Habits.*

1. Are you a sex offender?	(Reformed?)	(Why?)
2. Do you smoke or drink?	(Reformed?)	(Why?)
3. Do you swear?	(Reformed?)	(Why?)
4. Do you gamble?	(Reformed?)	(Why?)
5. Do you contract debts?	(Reformed?)	(Why?)
6. Do you pay debts?		
7. Do you lie?	(Reformed?)	(Why?)
8. Do you steal?	(Reformed?)	(Why?)
9. What do you consider your worst habits?		

VII. *Social History.*

1. Have you ever held an office in your community or church?
2. Do you belong to any secret societies? Ever held office?
3. Are you interested in community enterprises? (Good roads, etc.)
4. In what ways have you wanted to make your community better? Or are you satisfied to let every man take care of himself?
5. Have you ever been before a court on a charge? In jail?

VIII. *Grounds and Degree of Objection.* (If the objections are on religious grounds, follow A.; if on social or political grounds, follow B.)

A *Religious Objections.*

1. Degree of objection.
   a. Do you object to all wars or only to this war?

b. Would you take the military oath?
wear the uniform?
work in reconstruction?
do non-military work, such as farming?
enter a non-combatant military organization?

2. Specific objections.
   a. Do you object to war because your church forbids it?
      i. Why does it forbid it?
      ii. How long has it forbidden it?
      iii. Have any members of your church ever taken part in war?
      iv. Do you know any of your fellow-members who have been drafted? What did they do?
      v. Would you take part in a war to defend your religion?
   b. Do you object to war because the commandment says "Thou shalt not kill?"
      i. Do you literally obey the other commandments?
      ii. Do you consider all killing murder?
      iii. How do you account for divine sanction for war in the Old Testament?
   c. Do you base your objection on passages of Scripture found in the New Testament? What are they?
      i. Do you live up to the teachings of Jesus in all respects?
      ii. What do you think Jesus would do if he were drafted? Why?
      iii. What is your favorite passage?
   d. Do you object to war because you think that if you went to war and were killed, you would go to hell?
   e. Do you object to going to war because you are chosen of God not to fight with carnal weapons?
      i. Do you think it is wrong for me to fight?
      ii. How do you account for this war in God's plan for the world?
   f. Do you object to war because war involves hate? Makes deception legitimate? Impedes thereby the coming of the Brotherhood of Man?

3. Information concerning the History and Tenets of his Faith.
   a. How many members has your church as a whole ? where mostly?
   b. Who was the founder of your faith ?
   c. When were they first called by their present name ?
   d. What sacraments do they believe in ?
   e. Is the creed of your church written down outside the Bible?
   f. What does your church do to discipline disobedient members?
   g. What is its attitude towards the civil law ?
   h. Does your church believe in education ?
   i. Does your church co-operate with other religions ? Y. M. C. A.?

1. Degree of objection.
   j. Does your church permit marriage outside the church ?
   k. Does it believe in the Resurrection of the Body ?

B. *Objections on Social, Economic and Political Grounds.*

a. Do you object to all wars or only to this war ?
b. Would you take the military oath ? wear the uniform ? work in re-construction? do non-military work, such as farming? enter a non-combatant military organization?

2. Specific objections.
a. Do you object because this is a capitalistic and materialistic war? In what ways?
b. Do you object because the war is not worth the expense?
c. Do you object because the war is not getting Democracy, or rather is bringing about a German Autocracy, in this country?
d. Do you object to being drafted for a defensive war, or is your objection to being drafted and sent to Europe?
e. Do you object because the people did not vote on the war question?
f. Do you object because you refuse to be swayed by mob psychology and prefer to be in the minority?

3. Questions to determine how well grounded subject is in his views.
a. How long have you held these views ?
b. What social organization are you a member of ? How long? What is its peace program?
c. How did you come to be a Socialist ?
d. What books and magazines do you read ?
e. As a Socialist what do you consider your personal duty towards the war?
f. Do you believe that by staying out of the war you encourage your social brethren in Germany in a social revolution? Why has there not been such a revolution, already?
g. Do you believe in police force and enforced arbitration?
h. Do you claim protection under the law, the right to vote, freedom of speech ? Do you think that such liberties were obtained by war ?
i. Are you in accord with President Wilson's peace aims?
j. Why do not all Socialists oppose the war ?
k. What has been the attitude of Socialist parties here and elsewhere on past wars ?
l. What efforts do you make to spread the Socialist view ?

4. Questions to ascertain the extent of subject's information concerning Socialism.
a. What is the Socialist view as to the cause of the war ?
b. What are the Socialist war aims ? Peace program ?
c. What practical things have the Socialists done to end the war ?
d. What is the attitude of the German Socialist towards the war ?
e. Name some prominent Socialists here and abroad and give their viewpoints.
f. What is the Russian peace formula ? Attitude of Germany, England and France thereto?
g. What do you know about the Stockholm conference?
h. To what extent has the war furthered the cause of socialism here ?

THE VOWEL CHARACTER OF FORK TONES

By A. P. Weiss, Ohio State University

Introduction

Introduction. This set of experiments was suggested by a series of observations made by Koehler (2) in 1910 while he was investigating the motions of the tympanic membrane during auditory function. In the analysis of the photographic records of the vibrations secured by this method it was found that when the vowel ō (fold)[2] was sung at about 500 vibrations. the form of the sound wave approached a pure sine curve. At about 200 vibrations the sound wave for ō (fold) became very complex, but the partial corresponding to the frequency 500 became prominent while the fundamental 200 almost entirely disappeared. This fact led Koehler to conclude that pure tones had an attribute or property which he designated as '*Vokalcharacter*' and that this attribute changed in quality at intervals of about an octave. Thus the '*Vokalcharacter*' of the tone 265 is ū (so̤on), that of 526 is ō (fold) and that of 1066 is ā (far).

In going over Koehler's results in a limited way it was not possible to differentiate these vowels unequivocally. From his familiarity with the work of Meyer (5) the writer wondered whether Koehler had not merely made a more intensive study of what Meyer had described as 'quality' and which

[2] The designation of vowels will be in the Revised Scientific (National Education Association) Alphabet Key. **For those unfamiliar** with this alphabet short English words in parentheses indicate the sound of the vowel. For reference to key see (1) in list of references.

was regarded as a continuous series ranging from the 'mellowness' of the lowest tones to the 'shrillness' of the highest tones with intermediate degrees of mellowness and shrillness for the intermediate vibration frequencies.

Experiments were planned bearing on this question. One investigation aimed to determine the vowel character as judged by untrained or partially trained subjects to get some idea of the acuity of discrimination in general. The orientation seemed desirable so as to plan a second experiment using refined apparatus and specially trained subjects with an intensive technique. The experiment with the untrained subjects was started and finished while work on the intensive apparatus was progressing. The latter was brought to a stop by the war which made it impossible to secure the quality of tuning forks necessary for the work. The writer regrets that the intensive results as originally planned cannot be included in this study; but since there is no prospect of getting the forks and the apparatus within a reasonable time, the qualitative results are presented separately for the benefit of those who already have the facilities and interest for the quantitative aspect.

Statement of the Problem. The experiments were designed to reveal the extent to which tuning fork tones resembled the vowels used in language. As a supplementary phase of the experiment there was also opportunity for considering the terminological aspect of the psychology of audition. The teaching of the attributes or simplest discriminations in hearing presents considerable difficulty to students, a difficulty which is perhaps only due to ambiguous or unfamiliar terminology. The divergences between musical and psychological practices in describing auditory phenomena are so great that some investigators think it hopeless to try to reconcile them. Among acousticians themselves the situation is not much better. A random list of the terms used by various investigators to describe the attributes of audition are: pitch, quality, tonality, vocality, tone-color, intensity, duration, volume, brightness, extent, clearness and local signature. Does this list of a dozen terms mean that twelve attributes may be discriminated? No investigator would maintain this. Does any particular term (say quality) mean the same for all investigators? Again the answer is negative. Indeed the pessimistically inclined would maintain that any one of the dozen terms may mean any one of the other eleven.

Surely if experts cannot agree, then to learn the opinion of the naïve subject cannot add appreciably to the confusion. Whether the ability to detect a resemblance to vowels in fork

tones is best described by the term quality, vocality, tone-color, brightness, volume, or any other term, will be determined in part by the ease with which untrained subjects can consistently discriminate vowels. If more consistent results are secured when the subject is asked to discriminate the vowel character than when he is asked to discriminate the brightness, quality or tone-color, supposing these all refer to the same fundamental attribute, then obviously vowel character or vocality is much the better term to use.

To test the value of a term, untrained subjects may often be used more effectively than trained subjects with special habits and prejudices toward a terminology developed under circumstances deviating very widely from the average class room conditions under which psychology is taught. Because of the lack of agreement on terminology no attempt will be made to determine whether the discriminations made by the subjects are attributive or cognitive in character. It is maintained only that an habitual or practical method of discrimination is demonstrated to the degree that there is consistency in the vowels that are assigned to the various fork tones.

Since performing these experiments Meyer (4) has suggested the term *vocality* as a substitute for his earlier term *quality* and as a shortened form of *vowel character*. It is in this sense that we shall use the term vocality in this article. Titchener (8) seems disposed to adopt the term vocality in the sense in which Meyer suggests, but Titchener has retained the term *volume* which is regarded as an attribute independent of the vocality. Rich (10) regards *pitch* and *volume* as independent attributes, but does not indicate the relationship between vocality and volume.

Simply stated our problem is to determine how consistently relatively untrained subjects can discriminate vocality in tuning fork tones and what utility the term vocality has in describing a fundamental fact in audition.

I. Experiment with Elementary Students

Class Experiment

A class experiment was given the last week in November, 1913 at Ohio State University to 150 students mostly freshmen and sophomores taking the course in elementary psychology.

Experimental Method. Ten Koenig tuning forks mounted on resonance boxes were placed at the front of the lecture room. The vibration rates of these forks were 128, 256, 320,

384, 512, 640, 768, 896, 1024, 1152 double vibrations. The subjects were instructed as follows.

Each fork will be sounded twice. The first time it is bowed merely listen, eyes closed, and form a provisional judgment as to which vowel in the English language the tone most nearly resembles. The second time the fork is sounded make a final judgment and record a short English word which contains the vowel sound. The list of vowels and words on the board are to be regarded as merely illustrative. They do not include all the vowel sounds in the language nor all that may be used in reporting the vowel character of the fork tones.

The following is the list of vowels and words that was placed on the board:

e as in feel, *a* as in sale *o* as in tone *u* as in moon,
a as in far, *oi* as in boy, *yu* as in cure.

The tones lasted about three seconds. The recording was done on a narrow strip of paper, beginning at the bottom. As soon as the word was written it was folded out of sight. The next word was placed on top of the folded portion and it in turn was folded down. In this way there was no copying or referring back to previous judgments. In the presentation high and low forks alternated indiscriminately.

The term *series* as used in these experiments means a single presentation of each of the ten forks. A *discrimination* or *judgment* means that a fork tone has been sounded one or more times and that the subject has reacted to the tone by reporting, either orally or in writing, that vowel which in his opinion most nearly resembled the fork tone.

While performing the experiment it was noticed that the reactions were delayed and hesitating. Judging from the facial expressions the discriminations were difficult to make during the earlier part of the experiment but toward the end the reactions were more prompt.

After one week the experiment was repeated. This repetition is designated Series B; the original presentation as Series A. The second series was not expected by the students. The repetition was made to determine whether a second series would reveal greater uniformity and thus indicate some practice effect. The method was the same as for Series A. The results of both series are recorded in Table I. To enable direct comparisons with the subsequent experiments in which the number of subjects and the number of series are different, all results have been converted into percentages.

TABLE I

Class Experiment. Two Series

First Series = A. Second Series = B.

One hundred and fifty elementary students. The values in the table represent percentages. Thus the first value 48 means that for the fork 128 in Series A, the vowel ū (soon) was reported in 48 per cent of the 150 judgments.

Forks	128		256		320		384		512		640		768		896		1024		1152	
Series	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B
ū (soon)	48	86	10	18	12	20	8	16	3	0	6	0	5	..	10	0	8	0	2	0
iū (cure)	6	0	10	0	10	0	4	0	6	0	11	0	8	..	8	0	6	0	5	0
ō (fold)	8	12	14	34	11	19	8	34	10	0	7	0	8	..	6	0	5	0	7	0
oi (boy)	4	0	7	0	9	0	10	0	14	0	10	0	14	..	8	0	5	0	8	0
ā (far)	3	0	4	12	9	31	9	43	3	3	4	2	9	..	5	0	6	0	6	0
ai (fine)	1	0	3	0	9	0	15	0	24	29	22	25	18	..	14	33	16	42	18	31
ē (they)	4	0	22	34	9	27	14	7	10	35	7	20	10	..	14	34	9	12	12	13
ī (see)	15	0	15	0	14	0	15	0	14	33	14	50	15	..	17	33	21	46	18	53
failed	11	2	15	2	17	3	17	0	16	0	19	3	13	..	18	0	24	0	24	3

Last line 'failed' refers to the cases in which no judgment was given.
Average failures for Series A = 17 per cent; Series B = 2 per cent.
Average number of vowels assigned per fork: Series A = 8; Series B = 3.
In Series B, fork 768 was not available.

Table I. In Series A the failure to assign a vowel to the fork tones was nearly eight times as great as in Series B. The 17 per cent failures in Series A are not excessive however. If the same subjects had been asked to discriminate between ten degrees of saturation in colors, irregularly and successively presented, the failures probably would have been greater. That the failures decreased to 2 per cent in Series B indicates a rapid increase in skill with practice. In Series A the forks 128, 256, 512, 640 and 1024 have all of the eight vowels assigned to them, but some one of the vowels was used in more than one-fifth of the total number of judgments. In Series B the average number of vowels per fork was only three and every fork except 320 has more than one-third of the judgments given to some one vowel. This again implies a rapid increase in the uniformity of the reactions.

Nature of the Vowel Reaction. In Series A the percentage of ī (see) for all the forks was unusually large. The experimenter could not at first account for this fact. By bowing the forks gently the tones are so pure that no unpracticed subject would detect overtones and since the percentage was practically the same for all the forks it did not seem likely that the difficulty was in the source of the sounds.

It occurred to the experimenter that the subjects were not

only discriminating the particular vowel character of the tone but were also introducing an additional ī (see) vowel because they did not pronounce the vowel character of the fork tone orally. If for instance a fork whose vowel character resembles ō (fold) is sounded and an attempt is made to pronounce or sing the *o* silently, some consonant will be spontaneously supplied by the whispering and any vowel component of this consonant would be especially emphasized because of the instructions to differentiate the *vowel* character. Thus if the consonant *h* is supplied to the *o* to make *ho,* the *h* would not be differentiated as *h* because of this emphasis on the *vowel* character. If *h* is regarded as a semi-vowel, its silent pronunciation would resemble the oral pronunciation of *heee* and this in connection with the emphasized vowel *o* would give *heeeooo.* The *heee* would form a part of every fork tone and thus increase the frequency of the ī (see) reaction for all the forks.

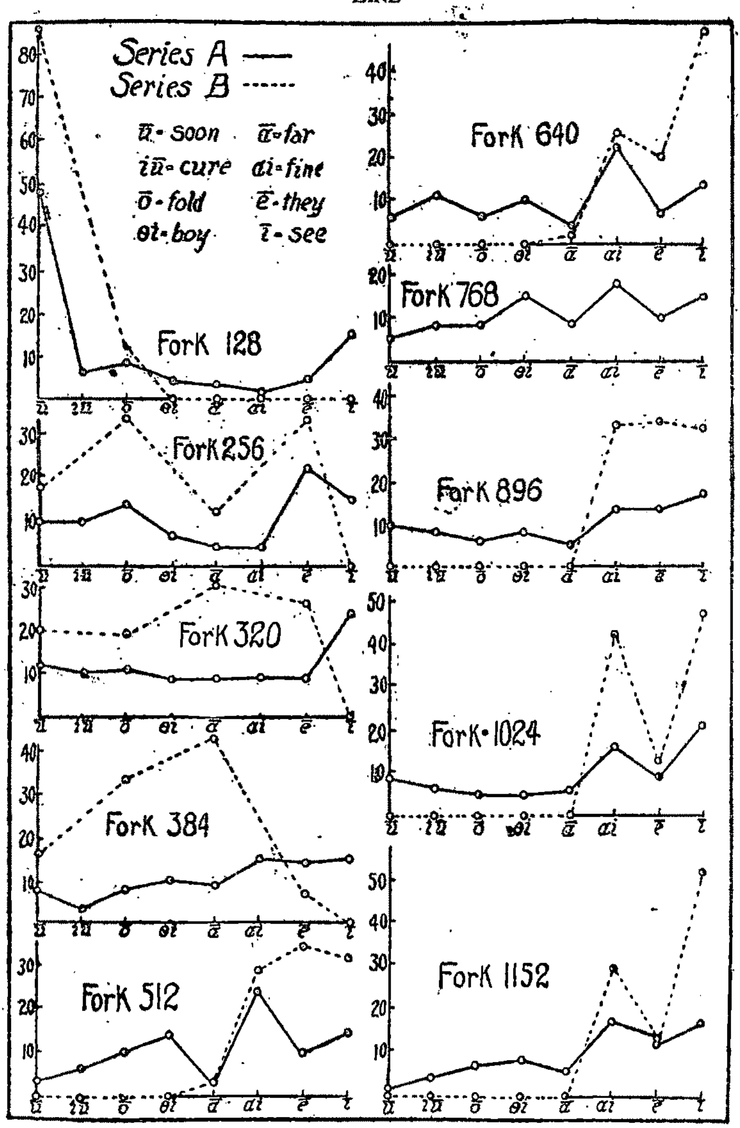
To restrict the analysis to the vowel character only, the experimenter, before Series B was given, called indirect attention to the consonants by stating that the vowel character of the fork tones may often be more easily analyzed if the subject will add consonants to the vowel sounds as is done in language. Thus we have pop, ish, peep, ho, ah, chirp, etc. It was hoped that by indirectly introducing the consonants their effect would be abstracted from the discrimination of the vowel character. The device seems to have been successful since the anomalous ī (see) distribution of Series A is absent in Series B. It was also pointed out that the sounds oi (boy) ai (fine), were not single vowels but diphthongs. This resulted in the elimination of oi (boy) and a restriction of ai (fine) to fewer forks.

Curves of Table I. The relation between Series A and B with respect to uniformity is best indicated by the curves. Each of the ten forks is plotted separately for both series. The vowels given on the base line are in the same order for all the forks.

If the vocality is independent of the vibration rates, the curves should approach the equation $y = k$ as a limit, approximately as is shown by the Series A curve for fork 320. Considering the Series A curves as a whole however, they would not be classed with the horizontal line type of curve. Series B is still further removed from the straight line type and the curves begin to give evidence of having definite maxima which in general augment the maxima of Series A. The maxima are toward the left for the lower forks; nearer the

CURVES I

ELEMENTARY STUDENTS. CLASS EXPERIMENT. VALUES TAKEN FROM TABLE I. EACH FORK PLOTTED SEPARATELY. VERTICALLY ORDINATE REPRESENTS PER CENT. VOWELS INDICATED ON BASE LINE



center for the intermediate forks; and toward the right for the higher forks.

The order of the vowels follows the series given by Koehler (3) : *m*, ū, ō, ā, ē, ī, the diphthongs not given by Koehler were interpolated.

Flexibility of the Vowel Series. In brief supplementary experiments not reported in this article it was found that when the subject is asked to determine the vowel character of a series, he usually assigns ū (soon) to the lower tones, and ī (see) to the higher no matter what the range of the series may be. There seems to be a tendency to condense or expand the vocality series to conform to the vibration limits of the tones used in the particular experiment.

If more intensive work will substantiate this it would mean that vocality is only partly dependent upon the absolute vibrations. Builders of musical instruments have adopted this partial dependence in the principle of 'voicing.' An organ builder may take two reeds both of 512 vibrations and voice one of them mellow and the other shrill, but he cannot voice the 512 reed so that it will be as mellow as a 128 or as shrill as one of 2048 vibrations. Similarity in voicing is restricted to a limit beyond which it is impossible to go. More space is devoted to this phenomenon under IX Discussion of Vocality.

II. Experiment with Laboratory Students

Individual Experiment [3]

To determine how the uniformity in vocality discrimination increases with subjects somewhat better trained in psychological observation, twenty-five series similar to those given the elementary class were given to six students taking the course in experimental psychology and two instructors in the department.

The ten forks were placed on a table in a room free from noise or other distractions and the subject sat about three feet away with his back to the forks. Before beginning, the nature of the experiment was explained and the subject instructed as follows:

The pure tone from the fork is to be analyzed as to its resemblance to the vowel sounds used in language. If a fork tone does not resemble any

[3] Mr. R. F. Bird, recently Fellow at the Carnegie Institute of Technology conducted the experiments with the laboratory students. His critical and painstaking tabulation of their data made it possible to add his results to those of Experiments I and III.

vowel with which you are familiar, the vowel or combination of vowels which most closely approximates the tone is to be given. The fork tones will be repeated as often as desired to enable you to make a judgment. The series of vowels on the card which has been given you is not to be regarded as complete, but only as indicating some of the sounds. Any other vowels may be used. If a vowel character seems to resemble two vowels, this is to be indicated by giving both vowels.

The following is the list of vowels and words that were placed on the card:

m as in hum,	*a* as in ball	*u* as in cue,	*a* as in sale,
e as in feel,	*o* as in tone,	*i* as in high,	*a* as in father,
i as in sit.			

when the subject stated that the fork sounded equally like two vowels, as ū-ō for instance, this was counted as one-half for each of the vowels. Such double vowels occurred in less than one per cent of the total number of discriminations, and since the number of possible vowel combinations is large, each combination occurred so few times that no attempt was made to tabulate them separately. The most frequent combination ā-ē occurred only fourteen times.

Five series were given at a single sitting of about a half hour. The order of sounding the forks was varied for each series so the subject could not anticipate the next tone. The recording was done by the experimenter on previously prepared data sheets, the subject merely calling out the vowel and word.

When the first five series had been finished for the six subjects, the results were tabulated and it was found that all the vowel sounds that had been given could be grouped under the following vowels:

um as in hum	*a* as in sale	*a* as in ball
u as in cue	*i* as in sit	*e* as in met
o as in hoe	*i* as in high	*a* as in cat
a as in father	*e* as in feel	*u* as in full

The eight vowels in the first two columns were placed on a new card. The four vowels in the last column were omitted because they had been reported in less than two per cent of the cases. The revised card was then used for the rest of the experiment and the subject was requested to restrict his judgment to the vowels given on the card unless none of these were adequate either singly or in combination to express the vocality. In justification of this restriction it was found that the first eight vowels were in common use by all subjects including those who made use of the four vowels omitted.

During the early trials the subjects reported that the num-

ber of vowels at their disposal seemed too limited to describe all the fork effects. This difficulty was partly due to the unequal emphasis on the intensity and duration. Thus e (set) ē (they) are the same vowel differing only in length. A little practice however, soon enabled the subject to make due allowance for such variations but the more critical observers maintained to the last that when, for instance, two different forks were both reported to have the ō (fold) character this did not mean that the two *o*'s were the same. It implied only that the differences between the *o*'s could not be described in a consistent and satisfactory manner. Under IX Discussion of Vocality, further consideration is given to this limitation in the vowel analysis of fork tones.

TABLE II

EIGHT LABORATORY STUDENTS—TWENTY-FIVE SERIES FOR EACH SUBJECT—TOTAL OF 200 DISCRIMINATIONS FOR EACH FORK—THE VALUES IN THE TABLE REPRESENT PERCENTAGES—THUS THE FIRST VALUE 55 MEANS THAT FOR THE FORK 128 THE VOWEL UM (HUM) WAS REPORTED IN 55 PER CENT OF THE 200 DISCRIMINATIONS.

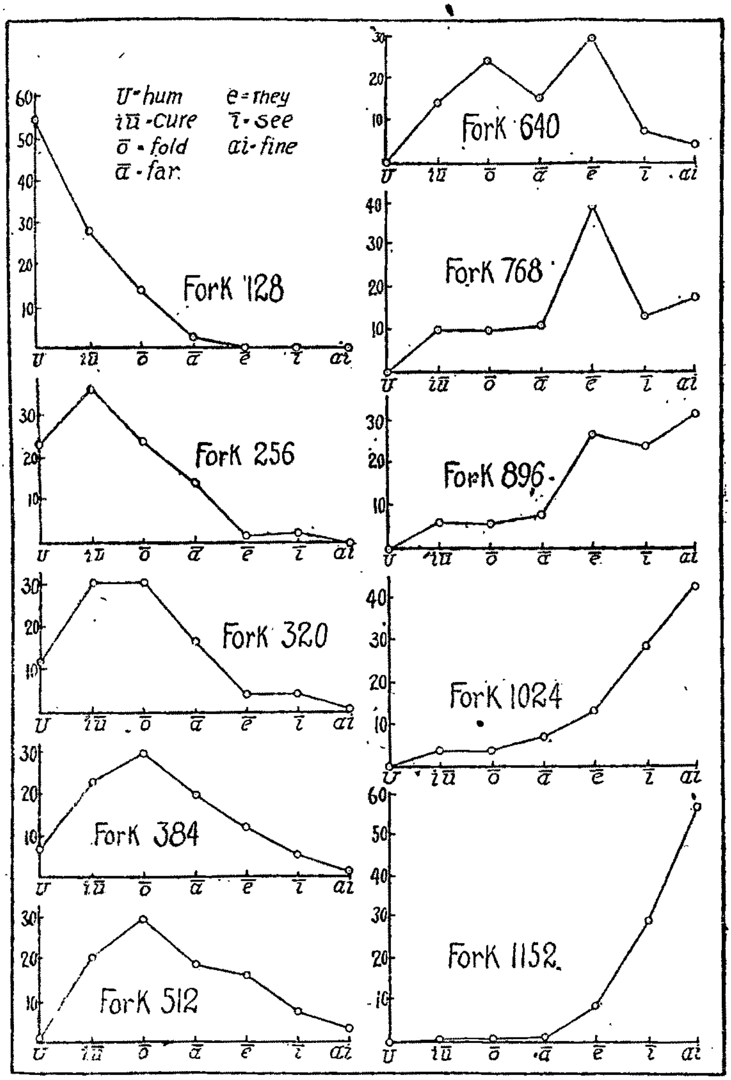
Forks	128	256	320	384	512	640	768	896	1024	1152
um (hum)	55	23	12	7	1	1	0	0	0	0
iū (cure)	28	36	31	23	21	15	10	5	4	1
ō (fold)	14	24	31	30	30	25	10	5	4	1
ā (far)	3	14	17	20	19	16	11	7	7	2
ē (they)	0	1	4	12	17	30	39	27	13	9
ī (see)	0	2	4	6	8	8	13	24	29	30
ai (fine)	0	0	1	2	4	5	17	32	43	57

Table II. There are no failures since each subject was free to have the tone repeated until he was able to assign some vowel. The percentages of any fork when read from top to bottom, or for any vowel when read from left to right, form a much more uniform series than for Experiment I with the elementary students. While the maxima are not as high as for Series B in Table I the geometrical relations between the series are more clearly shown. The maxima begin at one end for the lower forks (55 for 128) passing toward the center for the intermediate forks (30 for 512) and ending at the other extreme for the higher forks (57 for 1152).

The fork 640 seems to have two maxima, one at ō (fold) and the other at ē (they). This however does not interrupt

CURVES II

LABORATORY STUDENTS—INDIVIDUAL EXPERIMENT—VALUES TAKEN FROM TABLE II—EACH FORK PLOTTED SEPARATELY—VERTICAL ORDINATE REPRESENTS PER CENT—VOWELS INDICATED ON BASE LINE.



the progression except in a way that might be expected when the intermediate vowels are less clearly differentiated.

Curves for Table II. The plan of plotting the curves is the same as for Curves I. The graph for fork 128 has its maximum at the extreme left; for fork 1152 the maximum is at the extreme right; and for the intermediate forks it approaches either the left or right according as their vibration rates approach the upper or lower limits of the series.

On the whole the curves show a much more orderly progression than those for Experiment I. This would be expected from the more extended practice of the subjects and the change in the order of the vowel progression. In Experiment II the diphthong ai (fine) is placed after instead of before the vowel ī (see) as in Experiment I. While phonetically *ai* (fine) is a combination of ā and ī, yet since it ranks third in the order of frequency it seems probable that it is perceptually a unit for most subjects. However, the difficulty in placing this diphthong seems to indicate that there is a partial analysis.

III. Experiment with Teachers of Deaf Children

Class Experiment

To determine how consistently the vocality of fork tones could be discriminated by subjects trained in the analysis of vowel sounds, a class experiment similar to that given in Experiment I was performed before thirty teachers of the State School for the Deaf[4] at Columbus, Ohio. These teachers taught oral speech to children of all degrees of deafness. To detect and correct speech errors in those who cannot hear their own voices requires special ability in the analysis of vowel character and this ability should manifest itself even though the vowel character of the fork tones varies so considerably from the vowels of actual speech that the teachers are unable to use their special training to its limit.

The experimental method was practically the same as in Experiment I. Ten series were given instead of only one. The forks were not seen by the subjects. The tones were presented at random, each fork being sounded twice,—the first tone for orientation, the second for final discrimination. The subjects were instructed to listen to the tone and then record a word, the vowel of which resembled the fork tone. If no

[4] The writer takes this opportunity to thank Superintendent B. H. Jones for his interest and suggestions, and the teachers for their patience and conscientious effort during a trying hour.

vowel could be assigned nothing was to be recorded. Data sheets, ruled in squares with each square numbered were provided. At the top of the sheet were given the vowels:

um as in hum, *a* as in father, *o* as in tone, *a* as in cat
a as in sale, *i* as in fine, *u* as in cute.

The subjects were to regard this list as merely illustrative. Vowels not in the list were to be used whenever necessary.

TABLE III

THIRTY TEACHERS AT THE STATE SCHOOL FOR THE DEAF.—TEN SERIES—TOTAL OF 300 DISCRIMINATIONS FOR EACH FORK—THE VALUES OF THE TABLE REPRESENT PERCENTAGES—THUS THE FIRST VALUE 28 MEANS THAT FOR THE FORK 128 THE VOWEL UM (HUM) WAS REPORTED IN 28 PER CENT OF THE 300 TRIALS

Forks	128	256	320	384	512	640	768	896	1024	1152
um (hum)	28	18	17	11	5	7	3	2	3	2
iu (cure)	16	15	18	11	9	21	14	13	10	7
au (now)	0	3	3	0	1	0	0	0	0	0
ō (fold)	10	10	11	8	10	16	5	4	2	0
o (not)	1	2	1	2	2	0	2	1	0	0
ā (far)	29	22	21	15	11	6	11	5	5	4
a (fat)	1	1	4	6	10	3	3	3	3	3
e (set)	0	1	1	5	7	6	4	5	6	4
ē (they)	2	4	0	10	4	8	5	7	5	6
i (sit)	2	2	1	4	5	8	20	20	17	13
ī (see)	1	2	6	3	7	7	11	20	30	32
ai (fine)	0	2	2	1	8	4	7	8	4	11
failed	10	18	15	24	21	14	15	12	15	18

Average failures = 16 per cent.

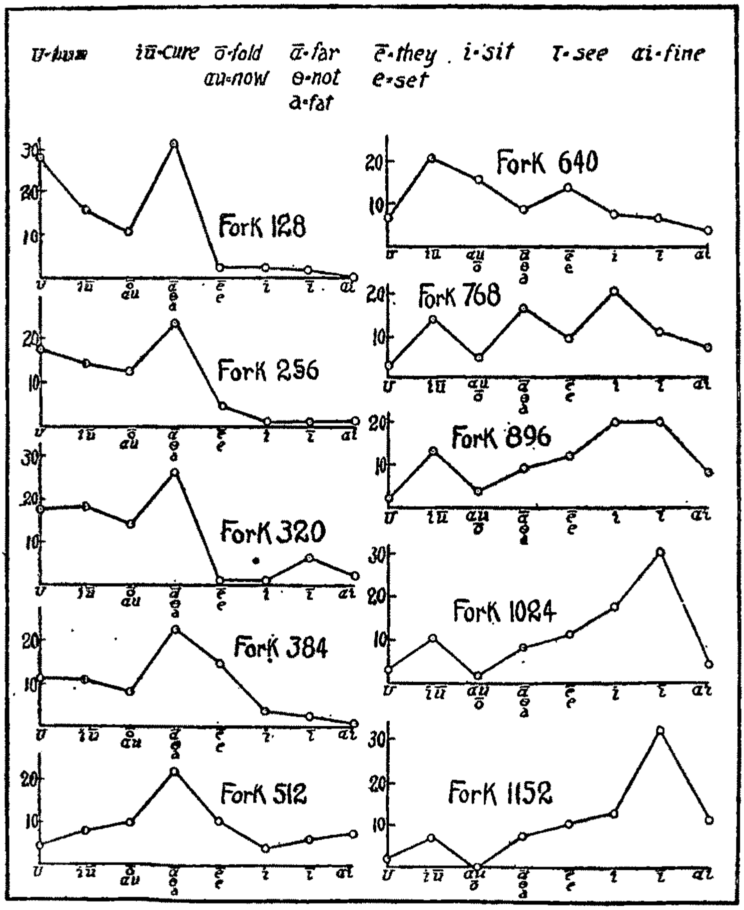
Table III. Perhaps the characteristic thing in the data of the teachers was the greater variety of vowels used, twelve as compared with eight for the Series A of the elementary students. The short vowels o (not), a (fat), e (set), i (sit), were used much more frequently than in the preceding experiments. The frequency of these short vowels is considerably less than of the long vowels, but there is clear evidence that the influence of the duration of the sound on the character of the vowel has been discriminated. This would be expected since the quantity of vowels is one of the important factors in teaching a non-hearing student to speak.

The relatively large percentage of failures was due partly to the instructions not to record the instances in which it was

impossible to decide upon the vowels, and partly to the novelty of being subjects in an experiment. The university students were more familiar with class demonstrations and laboratory work and the impressiveness of the occasion did not sit so

CURVES III

TEACHERS OF DEAF CHILDREN—CLASS EXPERIMENT—THIRTY SUBJECTS—VALUES TAKEN FROM TABLE III—EACH FORK PLOTTED SEPARATELY—VOWELS WHICH HAVE BEEN COMBINED ARE SHOWN UNDER EACH OTHER



heavily on their shoulders. Furthermore the attitude of 'trying to make a high score' was not so successfully eliminated with the teachers and this always has an inhibiting effect on the reactions of more mature individuals.

Curves III. To facilitate comparison with Experiments I and II the short vowels have been grouped with the corresponding long ones. The shapes of the curves correspond in general with the curves of Experiment I. The high frequency for ā (far) for the first five forks, seemed anomalous at first, but some of the teachers pointed out that this was the most ambiguous vowel of the series and where the discrimination was difficult this vowel served the purpose of a general utility vowel better than any of the others.

Considered as a whole the data may be regarded as complementary to the other experiments. It must be remembered that these teachers had not been trained for this particular type of discrimination. Their theories of vocalization, methods of teaching oral speech and past speech habits influenced their reactions to the auditory percept of the fork tones. Considering these factors it is rather remarkable that the results are as uniform as we find them. If these teachers had been asked to discriminate the series of fork tones into twelve degrees of quality, brightness or volume, with as little preliminary instruction as they received in discriminating the vowel character, the experiment probably would have failed.

IV. The Distribution of Vowels

To determine the differences between the vowel groups, the results of all three experiments were combined so as to include all the vowels given in Tables I, II, III. Such a combination will approach the average ability in vocality discrimination that would be expected from those subjects who are likely either as students or as teachers to exhibit a detailed interest in audition because of its practical or esthetic significance and to whose needs auditory terminology should be adapted if there are no scientific limitations. Thus if the term vocality is adopted it should not be difficult to explain to a musician what is meant by the ū-character of the lower end of the musical scale or the ī-character of the upper end.

No attempt is made to allow for differences in training, age, sex, experimental method and the variable number of series. Weighting separate experiments no matter how cleverly done, can never be a substitute for a uniform method in a single experimental plan. Yet since the aim is qualitative and the work

is regarded as preliminary to future intensive work, such a combination will give a conservative sketch that will help in developing a fruitful methodology.

TABLE IV

PERCENTAGE OF VOWELS ASSIGNED TO EACH FORK WHEN DISCRIMINATIONS IN TABLES I, II, III ARE COMBINED

Forks	128	256	320	384	512	640	768	896	1024	1152	Rel. Fr.
um (hum)	26	14	11	7	2	3	1	1	1	1	7
iu (cure)	44	25	25	17	12	16	14	10	8	5	17
au (now)	0	1	1	0	0	0	0	0	0	0	0
ō (fold)	12	21	19	21	15	16	8	4	3	2	12
oi (boy)	0	1	2	2	3	2	4	2	1	2	2
o (not)	0	0	0	1	0	0	1	0	0	0	0
ā (far)	13	17	21	23	13	8	11	5	5	4	12
a (fat)	0	0	2	3	4	1	2	2	1	1	2
ē (they)	2	14	9	12	16	18	19	20	10	10	13
e (set)	0	0	0	2	3	2	2	2	3	1	2
i (sit)	0	0	0	2	3	4	12	11	10	9	5
ai (fine)	0	2	3	4	14	12	12	18	24	27	12
ī (see)	3	5	7	6	15	18	14	25	34	38	16
Total...	750	726	732	698	705	721	584	730	729	709	

Total includes only the actual judgments. Percentages are derived from actual judgments made, failures not included.

TABLE IVa

DATA OF TABLE IV GROUPED INTO VOWEL CLASSES

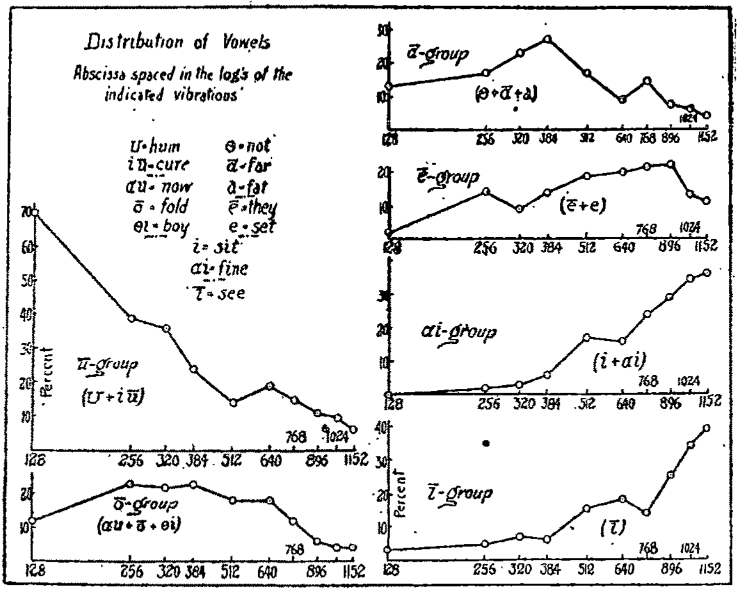
Forks	128	256	320	384	512	640	768	896	1024	1152	Rel. Fr.
ū-group	70	39	36	24	14	19	15	11	9	6	24
ō-group	12	23	22	23	18	18	12	6	4	4	14
ā-group	13	17	23	27	17	9	14	7	6	4	14
ē-group	2	14	9	14	19	20	21	22	13	11	15
ai-group	0	2	3	6	17	16	24	29	34	37	17
ī-group	3	5	7	6	15	18	14	25	34	38	16

Tables IV and IVa. All the vowels that were assigned in any of the experiments are given in Table IV. The last line marked 'Total' indicates the actual number of judgments only. If the failures had been included the total for each fork would have been 800. The low total of 584 for fork 768 is due to the fact that this fork was not available for Series B of

Experiment I. The last column (Rel. Fr.) gives the relative frequency of the vowels in per cent. Only six vowels have a frequency greater than ten per cent and these are selected as the more important classes. In Table IVa those vowels having a frequency of less than ten per cent are grouped together with those of the six important vowels to which they seem to belong. This gives the six *groups* of vowels shown in the first column. Each of these groups has a frequency that is large enough to determine the approximate vibration rate where the maximum frequency for a given vowel group lies.

CURVES IV

COMBINATION OF EXPERIMENTS I, II, III. SHOWING FREQUENCIES OF THE VOWEL GROUPS AT THE DIFFERENT VIBRATION RATES AS TAKEN FROM TABLE IVA.



Curves IV: The curve of the ū-group shows its maximum very decidedly at the fork 128. For the ō-group no decided maximum appears although its position is probably between 256 and 384 vibrations. The ā-group has a decided maximum at 384 and the drop on either side seems to justify the assumption that its highest frequency is more toward the higher forks than the ō-group curve. The ē-group curve is somewhat am-

biguous but in the reverse sense of the ŏ-group, its maximum seeming to lie between 512 and 896. The ai-group curve is similar to the ī-group; the maximum is at the highest fork used in the series, but shows evidence of having reached its highest frequency. For the ī-group the maximum is also with the highest fork but the slope of the curve at this work indicates that the point of highest frequency has not been reached.

A general consideration of the curves substantiates the statement made by Titchener (8) that the lower tones have a definite ū-character and the higher tones a definite ī-character.

V. The Consistency Between Vowels

The consistency in making the discriminations in these experiments approaches two limits:

1. When the consistency is least a subject will assign any vowel taken at random from his repertory of vowels every time he is asked to report the vowel character of any tone.

2. When the consistency is greatest a subject will assign the same vowel to the same fork every time it is presented.

The consistency may be measured by squaring the number of times a given vowel is assigned to a given fork. Thus if out of ten trials with fork 128 a subject reports 'u' *five* times, 'o' *two* times, and 'um' *three* times, the sum of the squares of the frequencies (5, 2, 3) is $25 + 4 + 9 = 38$. The number 38 measures the consistency.

If the subject had reported only a *single* vowel for all ten times, this would give a frequency of 10 for one vowel and the sum of the squares (since there is only one vowel) is $10^2 = 100$. The value 100 represents the upper limit or maximum degree of consistency.

If the subject assigns ten *different* vowels, each vowel can occur only once and the sum of the squares is $10 \times 1^2 = 10$. This represents the lower limit or minimum degree of consistency.

To make the measure of the consistency independent of the number of trials, the sum of the frequencies of the vowels squared, is divided by the square of the number of trials. By doing this the maximum consistency becomes 1.00 no matter how many trials are given, but the minimum will depend upon the number of trials.[5] Since the vowels available are relatively

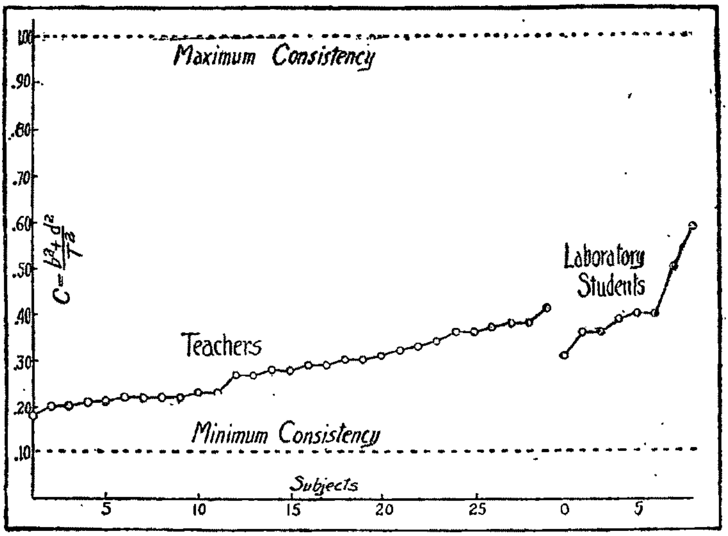
[5] The consistency (C) is algebraically expressed as follows: Let T represent the number of times a given fork is presented, and $b + d$ the number of times each vowel is assigned. Then $C = (b^2 + d^2) / T^2$.

few and both groups used the same vowels we can regard the minimum consistency as equal for both groups of subjects. For this we have selected $T = 10$ which gives the minimum consistency of .10.

Since there were only two series in Experiment I the consistency was not calculated.

CURVES V

INDIVIDUAL DIFFERENCES BETWEEN SUBJECTS AS MEASURED BY THE CONSISTENCY OF THE VOWEL DISCRIMINATIONS—SUBJECTS ARRANGED IN ORDER OF MERIT



Curves V. The dotted horizontal lines indicate the maximum and minimum limits of consistency. Each small circle on the intermediate curves represents one subject, and these have been arranged according to their degree of consistency. The vertical coordinate represents the average consistency for all the forks.

Considering the curve for the teachers, we find it relatively flat and that it does not indicate great individual variations. The median is at about 28 and this means that on the average

If the same vowel is assigned each time $(b^2 + d^2) = T^2$ and $C = T^2 / T^2 = 1.00$, the *maximum consistency.*

If a different vowel is assigned each time $(b + d) = T$ or $C = T / T^2$ the *minimum consistency.*

each subject assigned about three different vowels to each fork. For the laboratory students the median is at 40 indicating that on the average two different vowels were assigned to each fork. Out of a possible thirteen vowels actually used this represents a fair degree of reliability in making vocality judgments. If only the vowel *groups* indicated in Curves IV had been used in the measurement of the consistency it would have been more than twice as great and the curves would lie about half-way between the maximum and minimum limits.

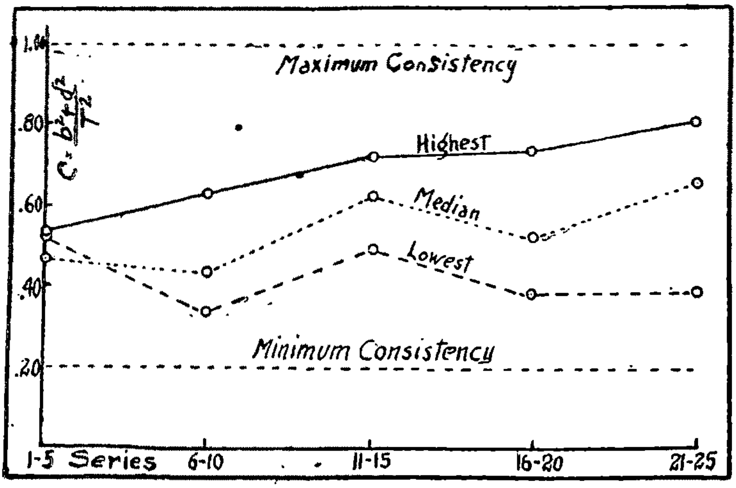
The individual differences between subjects conform to the usual results of those psychological experiments in which there is a gradual transition from one degree into another as for instance in auditory acuity. There is no evidence of a tendency toward a separation into two classes as into musical and non-musical subjects.

VI. Effect of Practice on the Consistency

In the Series B of Experiment I the consistency is much greater than for the Series A. The question arises what is the effect of practice upon the variability of the discriminations? In Curves VI the consistency for three subjects from

CURVES VI

Practice Effect in Vowel Discrimination—Three Subjects From the Laboratory Class—The Consistency is Calculated After Every Five Series



Experiment II was calculated for successive groups of five series each. The curve marked 'highest' represents the subject whose average consistency for the twenty-five series was greatest. The 'median' curve represents the subject who stood intermediate, and the 'lowest' curve represents the subject whose discriminations varied the most.

The curve for the 'highest' subject shows a gradual increase in the uniformity with each succeeding group of five series. The rise is not marked and signifies that a high degree of consistency had already been reached after the fifth series. For the 'median' subject there is a slight oscillation which probably implies that the consistency is no longer a function of training or practice after the first five series. This is more clearly shown by the 'lowest' subject whose curve shows even a slight drop between the fifth and twenty-fifth series.

The curves seem to indicate that the ability to discriminate vowels in tuning fork tones reaches a constant level after less than five series or after fifty discriminations. This of course does not mean that the maximum acuity has been attained. It merely indicates that the subject has isolated the vocality factor and that he is not confusing it with pitch, intensity, etc.

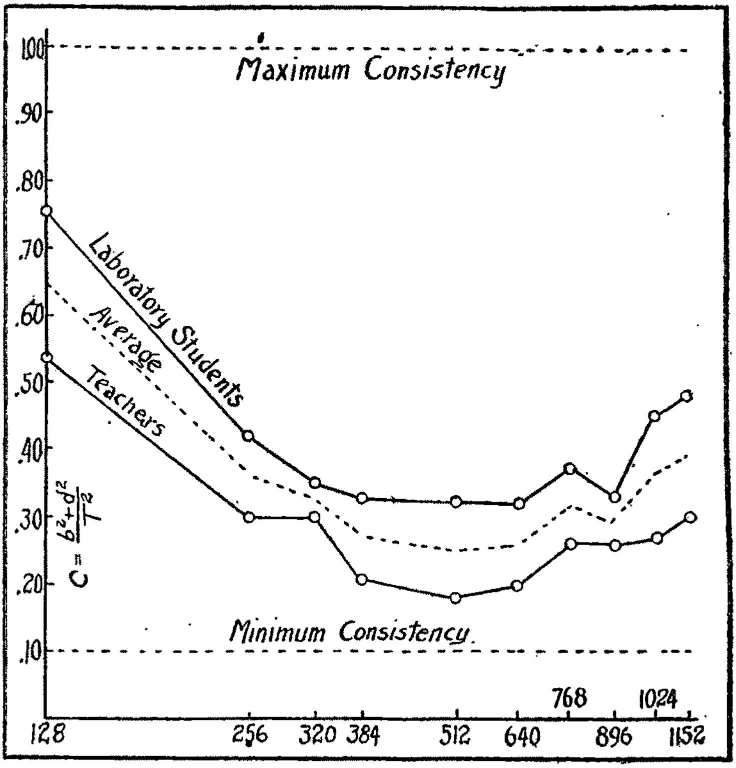
VII. The Consistency Between Vocality and Vibration Rates

The preceding topic indicated the individual differences between subjects. In Curves VII the individual differences of the forks, so to speak, are shown. The nearer a point on the curve approaches the dotted line indicating the minimum consistency the greater is the number of vowels assigned to it. To show the octave intervals as equal, the base line has been *spaced* in the logarithms of the vibration rates, although for easy reading the absolute vibration rates are given.

The curves clearly reveal that the forks in the middle region are more variable than the extremes. This is the opposite of pitch relationship or tonality which shows finer discriminations in the middle regions. The average consistency for the forks is indicated by the dotted curve. In consideration of the fact that these curves are based upon thirteen vowels, the degree of consistency is rather high. If only the vowel *groups* had been taken the curves would have been shifted upward to a median position between the limiting degrees of consistency and this would imply that on the average not more than two vowel groups would be reported for any one fork.

CURVES VII

RELATION BETWEEN VIBRATION RATE AND NUMBER OF VOWELS DISCRIMINATED—SUBJECTS FROM EXPERIMENTS II AND III



VIII. COMPARISON WITH THE WORK OF KOEHLER

In comparing the results of our experiments with those secured by Koehler it is necessary to allow for the different conditions under which the data was secured. Our comparison curve is a combination of three separate experiments. Koehler's curve is the result of theoretical deductions drawn from his own experiments and various other sources. Along the base of the Curves VIII the vibration rates of the forks are shown, spaced in logarithms.

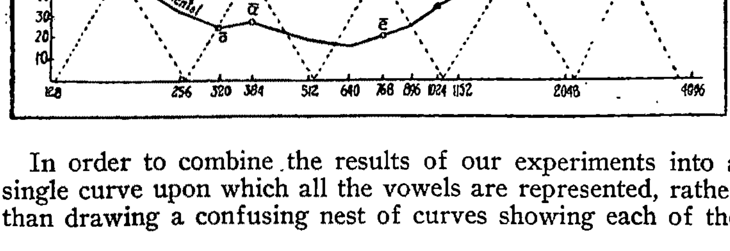
The dotted oblique lines represent Koehler's theoretical assumptions on the principle that each vowel has a maximum

frequency at some absolute vibration rate, with the frequency receding on either side until at the adjacent octaves its frequency is zero. These adjacent octaves however are the maxima for other vowels. Thus Koehler gives the vowel ū (soon) at 265; that is the vowel character of a pure tone of 265 vibrations is ū (soon) and under ideal conditions this vibration rate would be characterized as ū (soon) in 100 per cent of the judgments. As the vibration rate is increased, the subjects would characterize the tone as ū with a trace of ō. A further increase in the vibration rate would make the ō component stronger until a rate is reached at which the vocality resembles ū and ō. If at this vibration rate, which is indicated by the points at which the oblique lines cross, a number of subjects are required to report which of the two vowels ū or ō the tone most resembles, half of the subjects would report ū and half would report ō.

As the vibration rates are increased from this point the ō component becomes stronger until at 526 there would be 100 per cent ō judgments. In the same manner, as the vibration rate is decreased from 265 the vowel character approaches um (hum) until at 132 we have 100 per cent um judgments but none of ū. Whether or not the maxima should be connected with the respective minima by straight lines, Koehler leaves open for the present.

CURVES VIII

Comparison of the Experimental Results With Koehler's Theoretical Assumptions—Base Line is Spaced in the Logarithms of the Vibration Rates



In order to combine the results of our experiments into a single curve upon which all the vowels are represented, rather than drawing a confusing nest of curves showing each of the

forks separately, we have retained only the maximal points of the separate vowels. For the intermediate forks we have taken from Table IVa the average of both the percentages involved. For instance, the maximum for the ū-group is at fork 128 and the per cent is 70; the similar fork for the ō-group is 320 and the per cent is 22; at the intermediate vibration 256 the per cent for the ū-group is 39 and for the ō-group 23. The average of 23 and 39 is 31 which is the value we have selected for our experimental curve at 256. For all the other forks, which were not themselves the maximum for a vowel group, the same method was adopted.

Whenever untrained subjects or general class experiments are used, there is usually a masking of those effects that would be revealed by more intensive methods. This masking may be so complete as to be a virtual obliteration. If however there are *relative* maxima and minima shown in the graphical representation of the qualitative results these should coincide more or less closely with the quantitative experiments just as the relative maxima and minima of the quantitative experiments should coincide with the theoretical limits of ideal conditions if the theory represents the facts.

From the experimental curve it will be seen immediately that there is only one decided maximum at ū (soon) and a less distinct one at ī (see). Our ū coincides with Koehler's *m*, but none of the other vowels correspond with Koehler's nor is there any evidence of symmetry in their distribution. By shifting our curve to the right one octave all of our vowels are brought within Koehler's octaves. But whether this rather strenuous stretching is justified by the difference in language habits (English as compared with German) will remain doubtful until this factor is itself made the basis of a special study.

IX. Discussion of Vocality

Relation between Vocality and Vowels. Perhaps the striking thing in making vocality discriminations is the fact that the fork tones do *not* sound like the vowels used in language but yet may be said to resemble them so closely that consistent judgments are soon made upon this basis.

Meyer regards the vocality as a continuous series limited on the one hand by the degree of mellowness of the lowest tones and on the other by the degree of shrillness of the highest tones; following this conception we may regard the vocality of the intermediate vibrations as varying combina-

tions of mellowness and shrillness. However to ask a subject to differentiate *degrees* of mellowness and shrillness directly, does not result in consistent reactions nearly so rapidly as when he is asked to discriminate the vowel character. Obviously this can only mean that the subject is not so familiar with the terms mellowness and shrillness as he is with the vowels of language.

Conventionalized Vocality Combinations. The vowels may be regarded as special names that have been given to those combinations of mellowness and shrillness that have been conventionalized into language elements. The vowel in actual speech is an auditory *complex* and its particular combination of mellowness and shrillness is only one of the components—that component however from which the vowel gets its name. There are of course many more combinations of mellowness and shrillness than manifest themselves in the vowels of any one language, but in speech an infinite number of vowels would be confusing and unnecessary. A dozen vowels meet the practical requirements of any one language. Convention and usage have determined which of the combinations shall be selected and the use of these has developed a degree of facility in their discrimination which gives them a predominance over all other possible combinations of mellowness and shrillness that are not used in language. The relative distinctness between vowels is thus merely an expression of the fact that we have selected and conventionalized those combinations which were most easily discriminated.

If the experimental conditions are such that the subject is asked to discriminate combinations of mellowness and shrillness that have not become conventionalized through their being a component of the language vowels, he describes them by the nearest of the conventionalized combinations (one of the vowels in his particular language) to which he already has learned to react. We might expect further that the vowels are rather closely related to absolute vibration rates because on the whole the range of the *average* human voice is rather restricted.

The Intervals in Vocality. The octave relationship which seems to exist between vowels may be regarded as a manifestation of the Weber-Fechner law. If a continuous physical series such as the frequencies of sound waves is to be divided into definite sensorial steps such as the vowels, the division will occur in equal logarithmic intervals rather than in equal absolute vibration intervals because the logarithmic intervals will give sensorial steps in which the dissimilarity between

adjacent steps is equal and greatest. That is, the equal logarithmic steps would become conventionalized rather than would the equal absolute vibration steps. This of course, would give a kind of an octave relationship for the given language. In other languages the apparent octave relationship may be based upon entirely different combinations of mellowness and shrillness. From the vowels and semi-vowels of all languages we might be able to construct a practically continuous series of all possible combinations of mellowness and shrillness.

Definition of Vocality. In the interest of uniformity in terminology the following definition or description of vocality is suggested:

Vocality is that attribute of auditory sensation which is described by the vowel and semi-vowel character of tones and noises. It may be regarded as a continuous series ranging from the degree of *mellowness* of the vibration rates at the lower limit of hearing to the degree of *shrillness* of the vibration rates at the upper limit. Various combinations of mellowness and shrillness manifest themselves as a resemblance to the vowel and semi-vowel sound: *m* (hum), ū (soon), ō (fold), ā (far), ē (they), ī (see), s, f, H, of which *m* is the mellowest and H [pronounced as the *ch* in *loch* (Scotch) or mi*ch* (German)] is the shrillest.

This definition or description is eclectic and is derived from suggestions of Stumpf, Meyer, Koehler and Titchener. If all investigators could agree to call this one property *vocality* and nothing else, and if they would exercise some restraint in the coining of new terms until they had assured themselves that they were describing a form of discrimination that had not been made by others, greater uniformity in terminology would result. If for instance, it is impossible to discriminate between two attributes of sensation then we have only one attribute and there is no advantage in giving this attribute two or more different names. If brightness (*Helligkeit*) cannot be discriminated from shrillness, then there is no difference between brightness and shrillness and one of the terms is sufficient. Which of the two terms will be adopted is of course immaterial. Priority in use is the simplest criterion.

X Summary and Conclusions

The problem was to determine,

A. How consistently relatively untrained subjects could discriminate the vocality or vowel character of tuning forks ranging from 128 to 1152 vibrations.

B. Whether the term vocality or vowel character is a suitable one to describe an attributive or cognitive fact of audition.

A. Consistency of Vocality Discrimination

From experiments with the elementary students (I)[6] we find even with only a single series that the ū (soon) character is predominant for the lower forks and the ī (see) character is predominant for the higher forks. The intermediate vowel classes, ō (fold), ā (far), ē (they), do not seem to have dominant positions at any particular vibration rate; but there is no question that they occur as a series without the possibility of interchanging their relative positions.

The results from the experiments with the laboratory students (II) show the same thing but in a more orderly manner. The discriminations of the teachers for the deaf (III) approach the results of the more specifically trained laboratory students (II) as well as might be expected when we consider the larger number of vowels used.

When the findings of all the experiments are combined (IV) and the vowels are reduced to six groups, each group has a relative frequency (Table IVa) which makes it probable that the group of vowels are associated in an orderly manner with corresponding vibration rates, but our experiments do not exhibit an octave or interval relationship (VIII).

There is no evidence of marked individual variation (V) between subjects in their general ability to discriminate vocality such as is supposed to exist between musical and non-musical subjects. A high degree of consistency is reached after relatively few vowel discriminations. For the elementary students (I) a marked improvement is shown after only one series. For the laboratory students (VII) the degree of consistency at the end of the fifth series (a total of fifty single discriminations) has practically reached the limit that is reached after 250 discriminations. The consistency of the vowel discriminations for the different forks (VI) is highest for the extremes and lowest for the intermediate forks.

B. The Term Vocality

From the rapidity with which the discriminations of vowel character become consistent (I, VI) it seems evident that when a subject is asked to discriminate the vowel character of a fork tone, the reaction is one that is already well estab-

[6] The roman numerals in parentheses refer to the headings from which the conclusions are derived.

lished by the ordinary (speech) habits of the individual. Practice (I, VII) improves the vocality discriminations so rapidly that no extended demonstrations are necessary to bring out the meaning of the term.

Improvement in the discrimination occurs spontaneously (III) and manifests itself by the addition to the vocality series of those vowels which are formed by lengthening or shortening the vowel sounds. Even when the discriminations become consistent the subjects (II) still report that while two tones of different vibration rates may be classed as resembling the same vowel there is still a difference between them which can be described as an admixture of another vowel sound but the degree of admixture cannot be reported with a high degree of consistency.

As a definition or description of the term vocality (IX) the following is suggested:

Vocality is that attribute of auditory sensation which is described by the vowel and semi-vowel character of tones and noises. It may be regarded as a continuous series ranging from the degree of *mellowness* of the vibration rates at the lower limit of hearing to the degree of *shrillness* of the vibration rates at the upper limit. Various combinations of mellowness and shrillness manifest themslves as a resemblance to the vowel and semi-vowel sounds: *m* (hum), ū (soon), ō (fold), ā (far), ē (they), ī (see), s, f, H, of which *m* is the mellowest and H, pronounced as the *ch* in lo*ch* (Scotch) or mi*ch* (German) is the shrillest.

REFERENCES

1. Funk & Wagnall's *Standard Dictionary*, 1913, page xxxviii.
2. Koehler, W. Akustische Untersuchungen I. *Zeitsch. f. Psychologie*, 1910, Vol. 54, p. 241.
3. Koehler, W. Akustische Untersuchungen II. *Zeitsch. f. Psychologie*. 1910, Vol. 58, p. 111.
4. Koehler, W. Akustische Untersuchungen III. *Zeitsch. f. Psychologie*. 1915, Vol. 72, p. 2.
5. Meyer Max F. On the Attributes of Sensation. *Psychol, Rev.* 1899, Vol. 6, p. 506.
6. Meyer, Max F. Zur Theorie der Geräuschempfindungen. *Zeitsch. f. Psychologie*. 1903, Vol. 11, p. 247.
7. Meyer, Max F. *Vorschaege zur akustischen Terminologie. Zeitsch. f. Psychologie*. 1914, Vol. 68, p. 115.
8. Rich, G. J. Preliminary Study of Tonal Volume. *Jour. Exp. Psychol.* 1916, Vol. 1, p. 13.
9. Stumpf, C. Ueber Neuere Untersuchungen zur Tonlehre. *Bericht über den VI. Kongress für Exp. Psychol.* Leipzig 1914, p. 305-344.
10. Titchener, E. B. *Text-book of Psychology*. 1912, p. 94-95.
11. Titchener, E. B. *Beginners' Psychology*. 1916, p. 52.

THE VOCALITY OF FORK, VIOLIN AND PIANO TONES

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Problem—The immediate purpose of this study has been to investigate the validity of the theory of absolute vocality as it has been set forth by Köhler and others, when applied to tuning forks, piano and violin. If the assumptions as made be true, then we may expect to find a *relative* degree of consistency between the several instruments. A tone of a given pitch should according to the theory have the same vocality, whatever the instrument that produces it, provided the vocality of the over-tones is considered. Köhler[2] assumes first of all that vowels belong to definite frequencies. Willis and other investigators also found that vowel character is dependent on frequency. Köhler further assumes that all vowel characters may be defined in terms of the proportion of ū, ō, and ī quality. In other words, these three are most definitely located with ū at the lower end of the scale, ī at the upper and the pure ō sound midway between. As one leaves either end of the scale, the vowel quality characterizing that end becomes less and less prominent and that of the other end increases. Those vowel sounds which his observers definitely located, m, ū, ọ̄, ā, and ē, were of such frequencies that their pitches represent successive octaves in the order named.

Observers—The observers were children from a first grade intermediate school, i. e., from the 7B to the 9A grades inclusive. The ages of the children ranged from 11 to 19, in numbers as follows:

No. of Children	*Age*
11	11
34	12
52	13
54	14
36	15
7	16
2	17
0	18
1	19
2	?

This made a total of 199 children with an average age of 13.3 and a median age of 13. Of these children, 123 had studied some little music outside of that regularly given in the public schools, and 76 had

[1] This research was done, under the direction of Dr. Weiss, at the Ohio State University.

[2] Köhler, Wolfgang, 'Akustische Untersuchungen, II,' Zeitschrift für Psychologie, 1911, 58, pp. 59-140.

not. No definite information could be obtained as to the amounts, so it was merely recorded as 'some' or 'none.' As a general rule, it meant a few lessons on the piano. In addition to the 199 children to whom the three series, i. e., forks, piano and violin, were given, 33 others were given the fork series and the violin series and 26 the fork series only. These results however are not included in the data herein given, as it was desired to have results from the same children for all three series.

Material—The material used consisted of a series of tuning forks including the frequencies 256, 512, 1024, 1280, a piano, and a violin. The experimenter was a performer on both these instruments and was thus able to handle them advantageously for such an experiment.

Method—The experiments were performed in a room in which there were tables and chairs across either end. Approximately twenty children were given the experiments at one time. They were seated at the tables in such a manner that their backs were to the experimenter who played the instruments. The position of each child was about ten feet from the source of the sound.

After the forks had been shown to the children and the method of obtaining a sound from them had been explained, the following instructions were given:

Listen carefully to each fork as it is played and then when I have finished with it, write down opposite the number (serial number, as 1. 2. 3., etc.) the word or vowel it sounds most like to you. I shall play each tone three times. Listen carefully till I have finished and then write down the word. If you can not think of one that exactly fits, put the best thing you can think of; if you can think of none, leave that space blank.

After it was certain that the children understood what they were to do, the first series was presented, giving 256, 512, 1024, 1280, and then a repetition of the same with the order reversed. The same tones were then given in the piano and violin series with the addition of 384 preceding and 1536 following the series as given above. An interval of two or three seconds was allowed after the one tone rung off before it was repeated. Between tones of different pitch sufficient time was allowed to have each child note down the word or sound that he thought represented the tone just given.

The work was marked by interest and splendid cooperation on the part of the children. From the whole number tested, (more than 200) only a very few records had to be discarded because they showed lack of effort or a misunderstanding of instructions on the part of the observers. These were for the most part papers in which the children had tried to give the tonic-sol-fa equivalent for the tone given, which response is one of pitch rather than of vocality.

Results—Obviously in so large a number of records, a great variety of words will appear. The number of actual vowel sounds represented however is much smaller. For example, the words, moon, too, threw, blue and many others all represent the vowel sound ū. Likewise, the words, eat, me, seat, fiend and many others represent the sound ī. Among children we do not find the variety of vowel sounds that we do among adults. This is probably due to the narrower limit of the vocabulary of the child as compared with the adult, as well as the lack of development in finer discriminations of sound. It is often noted that children do not make the fine distinctions in the pronuncia-

tion of words that we find in the average adult. In this instance the sounds which they gave, fall rather naturally into twelve or thirteen groups. In the tables thirteen are given, but the numbers for one group, namely that with the sound short ŏŏ as in took, are very small. These vowel sounds and the number of observers giving such sound for each tone, are recorded in Table I.

TABLE I

NUMBER OF OBSERVERS, VOWEL CLASSES AND VIBRATION RATES FOR FORK, PIANO AND VIOLIN TONES

	ū	ō	ā	au	u	a	ā	ē	i	ai	e	ī	ŏŏ
Fork	who	know	saw	how	hum	fat	far	prey	hit	pine	get	feel	took
256	123	17	15	0	32	2	0	1	1	1	0	1	0
512	88	30	10	7	13	2	0	4	1	1	7	19	3
1024	38	45	4	9	6	1	2	10	10	3	13	45	0
1280	24	16	5	8	9	1	5	3	24	13	13	52	2
1280	17	21	8	8	10	2	4	4	23	18	7	59	0
1024	40	47	13	14	11	0	3	5	9	9	9	19	0
512	74	48	3	10	13	2	2	5	7	2	8	5	1
256	93	26	8	9	23	0	2	4	2	2	7	2	2
Piano													
384	49	56	4	5	13	3	3	4	3	1	3	8	1
256	62	61	5	4	14	2	0	3	0	5	4	4	0
512	15	18	7	4	17	2	6	11	11	11	7	44	0
1024	17	18	4	6	8	2	6	4	12	13	7	53	0
1280	7	18	4	2	8	2	2	7	18	18	10	54	0
1280	6	16	6	1	6	1	2	4	15	14	13	60	0
1024	33	29	8	6	12	4	3	6	8	12	17	11	1
512	29	37	13	4	22	4	4	11	1	4	8	6	1
256	39	59	11	3	17	0	2	2	2	7	7	3	0
1536	8	18	3	3	7	2	2	3	24	6	39	30	0
Violin													
384	23	44	13	6	27	1	3	10	4	5	15	14	7
256	47	57	12	7	21	2	2	11	4	1	2	9	1
512	15	28	3	3	9	3	2	9	7	13	20	61	0
1024	5	7	2	4	6	2	1	8	13	16	14	93	0
1280	10	9	2	5	11	0	3	5	22	20	6	78	0
1280	6	10	4	1	5	3	4	5	17	11	8	100	0
1024	16	15	7	8	11	0	2	13	19	9	14	49	3
512	23	48	15	5	16	1	3	19	8	4	12	14	6
256	33	60	14	11	17	3	6	7	1	12	5	1	4
1536	10	11	3	0	4	1	1	12	20	7	32	63	2

The numbers in the first column indicate the vibration rate of the tone used in each case. The numbers in the remaining columns indicate the number of times the vowel sounds at the top of the columns were given as corresponding with the tones indicated at the left.

For example, when tone 256 was sounded, from the forks, 123 observers reported the vowel sound u, 17 observers the vowel sound o etc. These figures are represented graphically in Figure I.

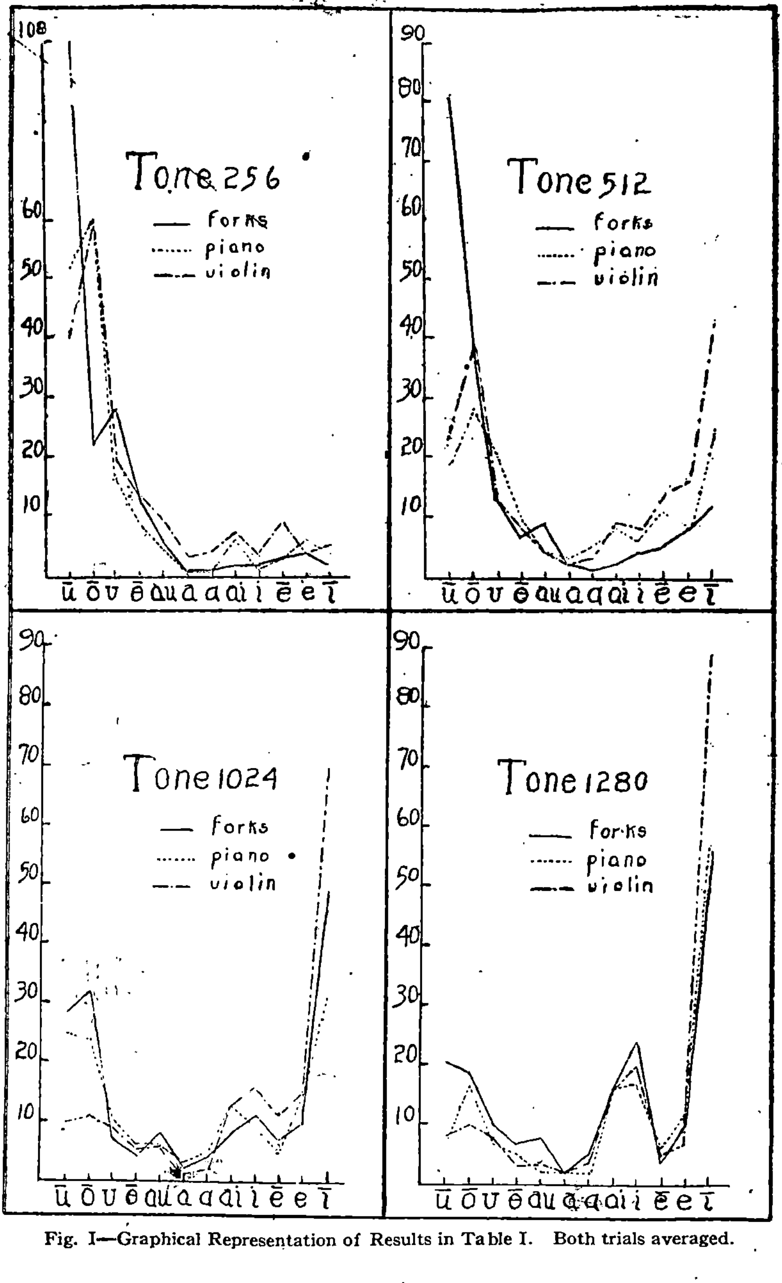

Fig. I—Graphical Representation of Results in Table I. Both trials averaged.

TABLE II

VIBRATION RATE AT WHICH VOWEL WAS MOST FREQUENTLY RECORDED

Vowel	Forks	Piano	Violin	Identity
ū (who)	256	256	256	3
ō (know)	1024	256	256	2
ā (saw)	256	512	256	2
au (how)	1024	1024	256	2
u (hum)	256	512	256	2
a (fat)	512	512 (1024)	256	2
ā (far)	1280	512	256 (1280)	0
ē (prey)	1024	512	512	2
i (hit)	1280	1280	1280	3
ai (pine)	1280	1280	1280	3
e (get)	1024	1024	512 (1024)	3
ī (feel)	1280	1280	1280	3
ŏŏ (took)	512	512 (1024)	512	3

The table reveals that eight of the vowel sounds were placed at the same tone for forks and piano; for eight the forks and violin agree. In six all three agree. Noting the number of times that any particular vowel sound was found throughout the whole series, we find that the numbers for ā, ā, and ŏŏ (Table I) are very small as compared with the other vowel sounds. These vowels might have been combined with other vowels but it was thought best to give actual results without any arbitrary attempt at grouping.

A very large majority of the observers agreed that the sound ū characterizes the tones of the lower end of the scale, and that the sound ī characterizes the higher. Between these limits there is more variation and diversity of opinion, than at the extremes. This fact is shown in Table III, in which is recorded the vowel sound which was given by the largest number of observers for a given tone.

TABLE III

VOWEL SOUND REPORTED MOST FREQUENTLY FOR EACH TONE

Tone	Forks		Piano		Violin	
	I	II	I	II	I	II
256	ū	ū	ū	ō	ō	ō
512	ū	ū	ī	ō	ī	ō
1024	ō and ī	ō	ī	ū (ō)	ī	ī
1280	ī	ī	ī	ī	ī	ī
384			ō		ō	
1536			ē		ī	

I and II refer to first and second trials. For fork 1024, vowels ō and ī were given by the same number of observers (45 each). For tone 1024 when sounded from the piano, ū was given by 33 observers and ō by 29.

It was thought desirable to calculate the consistency[3] or reliability of the vowels assigned to the various vibration rates. The formula $C = (b^2 + d^2)/T^2$ was used. For example, if tone 256 is sounded ten times, and the vowel *ū* is reported three times, the vowel *ō* four times, and the vowel *àu* three times, an application of the formula gives $C = (3^2 + 4^2 + 3^2)/10^2$ or reduced, $C = .34$. If the same vowel is given each time, we get $10^2/10^2$, a maximum consistency of $C = 1.00$, if a different vowel is recorded each time or by each observer we get $1^2 \ldots\ldots 10$ times or $10/10^2$, a minimum of $C = .10$. In Table IV are recorded the consistencies for each tone and for each instrument.

TABLE IV

CONSISTENCY FOR EACH VIBRATION RATE

	256	512	1024	1280
Fork	.34	.23	.14	.13
Piano	.17	.08	.09	.11
Violin	.15	.11	.17	.23

THESE FIGURES REPRESENT THE AVERAGE OF THE CONSISTENCIES AS CALCULATED FOR THE FIRST AND SECOND TRIALS—FIG. II SHOWS THE COMPARISONS MORE CLEARLY

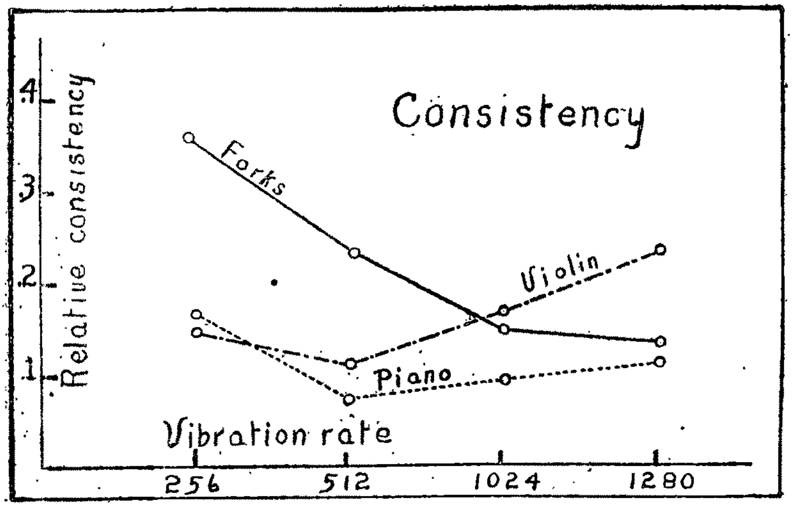

Fig. II—Graphical Representation of Table IV.

Among 200 observers, with all the possibilities of vowel sounds before them, it is not surprising that we do not get a greater con-

[3] In conformity with a similar article by A. P. Weiss it was thought best to adopt this formula.

sistency. We find that the curves of consistency for the forks and the piano are approximately the same shape, but in the curve representing the tones as given by the violin, there is a rise at the upper end, instead of the lower. Miller[4] has found that the lower tones when sounded from the violin have weak fundamentals. Furthermore, the resonance of the body of the violin is such that it emphasizes the higher tones and their partials. The lowest tones on the violin are lower than the fundamental resonance of the violin body. On the average, the consistency for the forks is greatest, the violin second, and the piano least.[5] The consistency or agreement between the two trials is also greatest for the forks, for the violin second, and for the piano least (Table III).

Another interesting result is that gained by comparing the vowels given for the two lowest tones and those for the two highest tones of the series. Fifty-three per cent of the observers gave either ū or ō as the vowel representing the vocality of the tone 384 as sounded by the piano, whereas 62 per cent gave these vowels for the vocality of tone 256. This fact shows a discrimination of the lower vowel quality of the tone 256 as compared with tone 384. Likewise, 37 per cent of the observers gave the five vowels representing the highest vowel quality,[6] when tone 1024 was sounded, whereas 52 per cent of the observers gave these high vowels when tone 1280 was sounded. This shows a discrimination of the ī vowel quality as the upper limit of the scale is approached.

In the violin series, 59 per cent of the observers gave the vowels of the lower end of the series after tone 384 was sounded, whereas 74 per cent gave these same vowels to the tone 256. This shows a discrimination of the lower vowel quality of the tone 256. Likewise, at the upper end of the scale, 50 per cent of the observers gave the ī, āī, or ī vowel sounds to the tone 1024, whereas 62 per cent gave these vowels to the tone 1280. This shows a discrimination of the increase of the ī quality as the upper limit of the scale is approached.

The addition of the two tones to the piano series and the violin series furnished a test of the children's ability to recognize the change in vowel character, when the change was not a part of the progressive series. A study was made of each individual's response to the change, i. e., whether he changed from a lower vowel sound to a higher, kept the same vowel for both tones, or changed from a higher to a lower vowel sound. These results are given in Table V.

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[4]Miller, Dayton, Clarence—The Science of Musical Sounds, 1916, p. 197.

[5]The question as to whether the consistency of different instruments changes for different vibration rates is an interesting problem, but one for which this data is insufficient.

[6] These figures were obtained by adding the number of au, e, ī, i, and ai vowels given for the tone 1024 (or 1280 as the case may be). These vowels are given the greatest number of times at either 1024 or 1280, when sounded from the piano. (Table II).

TABLE V

VOWEL CHANGES RECORDED WHEN TONE CHANGE WAS NOT A PROGRESSIVE ONE

Change	Lower to Higher	Same for Each	Higher to Lower
Piano			
From 256 to 1536	66%	18%	16%
From 384 to 256	18%	61%	21%
Violin			
From 256 to 1536	72%	21%	7%
From 384 to 256	19%	35%	46%

The results show that when the change in interval is from 256 toward 1536, a large per cent of the observers changed the vowel from lower to higher as would be expected. A small per cent of the observers kept the same vowel or changed from a higher to lower. On the other hand when the change in interval is from 384 to 256, (a small change when compared with the preceding one) a high per cent of the observers recorded the vowel quality of the two tones as the same, a small per cent recorded a change from higher to lower (the true change) or from lower to higher. The number recording a change from higher to lower was of course greater than those recording the opposite change. These comparisons are made on the basis of the piano series. In the violin series, the results are practically the same, except that in the change from 384 to 256, a higher per cent of the observers made a vowel change from higher to lower (the correct change) than those who kept the same vowel for both tones, as was the case in the piano series. This does not alter the conclusions to be drawn from these results however. The results simply mean that the observers discriminated a much greater change in vocality between tone 256 and tone 1536 than between tone 384 and 256.

Conclusions—The results of this study warrant the following conclusions:

1. There is unquestioned evidence that there is some degree of consistency in the vocality assigned to tones, whether produced by forks, piano or violin. This means that the vocality is a factor associated with pitch, but not one of absolute pitch.

2. The vowel quality ranges from the ū sound at the lower end up to the ī quality at the upper end of the scale, with a wide variety of vowel characters between the two, which become more and more like these extremes as the upper or the lower end of the scale is reached.[7] The great majority of the observers agree as to the location of the ū and the ī vowel sounds.

3. The degree of consistency of the vowels assigned to the various vibration rates is greatest when the tones are produced by the tuning forks. This is probably due to the absence of overtones, the tones from the tuning forks being relatively much simpler or purer than the same tones produced by the piano or the violin The vocality of any

[7] Titchener, E. B. 'Beginner's Psychology,' p. 52.

given tone depends not only upon the fundamental tone, but also upon the several overtones, and the intensity of each.

4. The results of this experiment agree with those of Köhler first, in that the vowel characters ū and ī are much the easiest to locate. Secondly, the location of the ō sound is somewhere near the same, 512 (Köhler located it between 520 and 530). Thirdly there is a gradual decrease of the ū quality and an increase of the ī quality as one proceeds from the lower to the upper end of the range. However these results do not agree with the assumption that the vowel character is dependent on frequency. Were this true the dominant vowel quality should be the same, whatever the instrument used. One of the principal differences obtained by using the several instruments, is the change in the number and intensity of the overtones present. We find that there is a shift in the location of some vowel characters with a change in instrument. Köhler reported that he had trouble with the observers, due to their unfamiliarity with the tuning forks. In the present experiments this was not true. Greater consistency was obtained with the tuning forks than with either of the instruments. Köhler suggests that the vowel character may be found not only at a given frequency, but at those frequencies which are in immediate proximity to it. He makes his theory of vowel character comparable in this wise to the resonance theory of Helmholtz.

No attempt was made in this study to point out a definite location for any given vowel sound, but the results do not point in that direction. The pitch of a vowel is not nearly so absolute as is assumed by Köhler. The order of vowels found as related to pitch range was indeed that obtained by Köhler, but the results would rather warrant the following assumption.

There is at the lower pitches a quality, which corresponds to the vowel ū of the speaking voice, and there is at the upper pitches a quality, which corresponds to the ī sound of the speaking voice. These might be referred to as a mellowness or a shrillness, as is done by Meyer, which we merely represent by means of the vowels. In between these limits there are many other characteristic sounds, which may resemble the ū or the ī or may be of such a character as to be identified with neither. These we indicate by means of other vowels which are formed in such manner as to have a mellowness or shrillness between the designated limits. The fact that, as Miller found, starting with the pure ō sound, one may by slowing down the movement of the graphophone produce a sound that becomes less like the ō and more like the ū, or by speeding up the movement, produce a sound that becomes less like the ō and more like the ī, does not necessarily imply that the vowel character is a function of the vibration rate of the fundamental only. The presence of upper overtones of high intensity will of necessity bring into the compound more of the ī element. This is shown particularly with the experiments on the violin tones. The presence of strong overtones whether high or low, will raise the vowel character. We accordingly find that when the tones are produced by tuning forks where the presence of overtones is comparatively unimportant, that the ū sound or quality predominates and that even with the higher pitches, the amount of the ī quality does not reach that of these same pitches when sounded from the piano or more especially the violin.

If a small mouthed bottle be held under a stream of water and allowed to fill, the tone produced by the water passing through the mouth rises in vowel quality as well as in pitch. This closing up of a larger cavity to a smaller produces the change from the mellow to the shrill quality, which we designate by means of the vowel equivalents. This is in some measure the same as what Watt[7] calls tone-volume, although he later defines vowels as "(partial) sounds of a somewhat indefinite pitch which lies about the point of range of pure tones of definite pitch where the greatest resemblance to these vowels is to be found." It seems, however, that it is not the slight variation in fundamental pitch that is so important, as the effect that the change in the intensity of the overtones has upon the whole, and thus upon the vowel character.

[7] Watt, Henry J., 'The Psychology of Sound,' p. 233.

RELIGIOUS BELIEF AND THE POPULATION QUESTION

By WESLEY RAYMOND WELLS, Colby College

In a previous article in this journal[1] I have spoken of the biological value of religious belief. Such considerations as were presented in that article ought to be supplemented by an account of one further situation, a very specific and concrete situation, in which religious belief manifests its biological value most strikingly. I refer to the correlation between religious belief and a relatively high birth-rate, especially in the case of belief of a somewhat legalistic sort such as is now best exemplified in Catholicism. This correlation is very significant. It shows that religious belief possesses survival-value of a high order.

The connection between religious belief and the birth-rate in ancient history is a well-known fact. Fustel de Coulanges[2] has pointed out how, in the patriarchal families of the ancient Greeks and Romans, religion required the continuity of the family. The fate of the ancestral spirits was believed to depend upon offerings made at their tombs by their descendants. Continuity of the family was required for the sake of the sustenance of the departed ancestors. Celibacy was an impiety, forbidden by religion and also by civil law when law arose out of religious requirements as something distinct from them. The ancient laws of Rome forbade celibacy.[3] The ancient Hindus had similar laws. For the Hindus as for the Greeks and Romans the extinction of a family caused the ruin of the family religion, and this was to be avoided by all means.[4] Among the ancient Hebrews also it was a fundamental religious duty to "be fruitful and multiply."

All this, however, is a matter of ancient history. It is more important to inquire about the relation between religious belief and increase of population in modern society.

[1] Vol. XXIX (1918), pp. 383-92.

[2] *The Ancient City,* (translation by Willard Small), Boston, 11th edition, 1901.

[3] See Cicero, *De Legibus,* iii, 2.

[4] See Fustel de Coulanges, *op. cit.*, p. 62.

In the first place, it seems a matter of common observation that, in general, the birth-rate is highest in families most influenced by religious belief. The reasons that might be assigned as the cause of this are various. In some cases the religiously inclined may feel a mystic obligation to rear a family. Fear of a concrete hell for those failing to fulfill their parental obligations may be the motivating power in other cases. Mr. Russell seems to incline to this explanation, and to think that the belief in hell-fire is thus biologically justified, when he says, " Men and women who can still believe the Catholic faith will have a biological advantage; gradually a race will grow up which will be impervious to all the assaults of reason, and will believe imperturbably that limitation of families leads to hell-fire "[5] A third possible explanation of the connection between a high birth-rate and religious belief is the partial identity of the parental instinct and the religious sentiment. According to McDougall,[6] the tender emotion, which is a correlate of the parental instinct, is a constituent also of the religious sentiment.

In the second place, statistical studies have supported the conclusions of casual observation by showing it to be a fact that, where religious belief thrives, there a relatively high birth-rate is generally to be found. The Italian premier Nitti [7] has called attention to this fact. Leroy-Beaulieu has made a statistical study of the situation, not only in France, but in various other countries as well. He has found a high birth-rate among the Spanish and Italian Catholics, and in those sections of France where the Catholic church is strong.[8] McDougall [9] accepts the contention of Benjamin Kidd [10] that one great influence of religious belief has been, and continues to be, its instrumentality as a support of the parental instinct against the tendency towards prudential limitation of the

[5] Bertrand Russell, "Marriage and the Population Question," *International Journal of Ethics,* Vol. XXVI (1915-16), p. 451.

[6] See Wm. McDougall, *Social Psychology,* pp. 66ff and Ch. XIII.

[7] Francesco S. Nitti, *Population and the Social Order* (translation), pp. 118-24.

[8] Paul Leroy-Beaulieu, *La Question de la Population.* See especially pp. 395-402. Leroy-Beaulieu gives the following explanation of the influence of religion, especially Catholicism, on population: "*La religion catholique, plus encore que toutes les autres enseigne la résignation à son sort, condamne l'égoïsme et déconseille l'ambition; c'est-à-dire, q'elle exalte le sentiment qui tend à rende les familles nombreuses et qu'elle reprouve ceux qui tendent à diminuer le nombre des enfants.*" Pp. 397, 98).

[9] *Op. cit.,* p. 272.

[10] See Benjamin Kidd, *Social Evolution,* p. 295; also Ch. V.

birth-rate with developing intelligence. Sidney Webb[11] explains the high birth-rate among Catholics in England by reference to the fact that the Catholic church absolutely forbids any regulation of the marriage state. Of the distribution of this high birth-rate he says,[12] "It is significant that Ireland is the only part of the United Kingdom in which the birth-rate has not declined; that in Ireland itself it has declined a little in semi-Protestant Belfast, and not at all in Roman Catholic Dublin; and that in the towns of Great Britain the decline is least in Liverpool, Salford, Manchester, and Glasgow—towns in which the proportion of Roman Catholics is considerable." Webb finds that among the metropolitan boroughs the highest birth-rate is "in those boroughs in which the Irish Roman Catholics (and the Jews who, in this respect, are in the same position) are most numerous."[13] Many economists have tried to correlate high and low birth-rates with conditions of poverty and wealth, but Webb shows that there is a closer correlation with religious belief than with economic conditions. As a matter of fact, however, poverty and religious belief are frequently found together since many people find compensation in religion for the lack of the material satisfactions of life.

For a most complete and convincing array of statistics upon this subject, Mr. Meyrick Booth's article, "Religious Belief as Affecting the Growth of Population,"[14] should be consulted. Booth shows, for example, that, according to the Catholic Year Book for 1914, the birth-rate per 1000 of Roman Catholics in Great Britain was 38.6, while the average rate for the whole population was but 24.0. In the United States, among those states which, according to the religious census of 1906, had a small population of Catholics, the birth-rate was low, averaging 15 or less per 1000. On the other hand, in five states with a large proportion of Catholics, the birth-rate averaged about 23 or 24 per 1000.[15]

There may be a temperamental basis for susceptibility to religious belief, and temperament is hereditary. As McDougall says,[16] basing his conclusions on studies made by Karl

[11] *The Decline of the Birth-Rate,* Fabian Tract, No. 131, 1907.
[12] *Ibid.,* p. 9.
[13] *Ibid.,* p. 9.
[14] In the *Hibbert Journal,* Vol. XIII (1914-15) pp. 138-54.
[15] Booth's article is a careful study of the situation, and gives many tables of statistics, which should be consulted by anyone who is not convinced of the importance of religious belief as influencing population increase.
[16] *Op. cit.,* p. 273.

Pearson,[17] about one-half of each generation is recruited from one-quarter of the preceding generation. The quarter with highest birth-rate is, as I have shown, the most religious portion of society. So far, then, as there is a temperamental basis for religious belief, and so far as temperament is hereditary, the religious temperament, and consequently religious belief, can not die out while the birth-rate is highest among those possessing the requisite temperament.

The correlation between religious belief and a relatively high birth-rate is most conclusive evidence of the biological value of religious belief. If unbelievers are not so much interested as others in founding families, then their unbelief perishes with them; while religious belief springs up in each new generation from a perennial source of vital human need.

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[17] See Karl Pearson, *Chances of Death.*

A NOTE ON PEN-LAPSES, INITIATED VISUALLY

By June E. Downey

The recent analysis by Dr. Roback[1] of a number of pen lapses, and, in particular, his observations relative to the possibility of a pen lapse being set off by the visual perception of a word already written leads me to record a number of self-observations made on pen-lapses occurring spontaneously during rapid composition.

My records were made immediately on recognition of the lapse. It was usually possible to call back the graphic cue and in other respects to snapshot the process.

As a preliminary to the report I may say that in composition my cue is very distinctly acoustic-vocal-motor. There are present, in addition, hand-motor sensations, since the writing movements involve considerable tension; and, also, a high degree of visual consciousness of what the written product looks like. Only under very great absorption in the content do I lose this consciousness of visual supervision. One consequence of this is that pen lapses rarely get past the visual censor; incipient lapses are apt to be detected in the making. Another consequence is, I suspect, the initiation of an occasional lapse through visual suggestion. I select the following cases for report, quoting directly with a few additions in parentheses, from notes made at the time the lapse occurred.

1. "Started to write 'coats' for 'goats.' After writing 'co,' I stopped and wondered what was wrong. The acoustic-motor cue was unambiguously 'goats,' but, visually, the 'co,' with a vague visual continuation of 'ats' seemed just as meaningful. There was considerable hesitation before correction was made....Definite split between the auditory-vocal-motor cue and the visual report on the process. Was the origin of the lapse grapho-motor in nature? (A more probable cause would seem to be the similarity in sound between the 'c' and the 'g.')......Noticed later that the word 'color' occurred in the line above, three inches to the left. Perhaps the sight of this 'c' suggested the lapse. 'Color-blindness' was the topic of the paragraph, so that 'C' may well have persisted in consciousness."

2. "Wrote 'world(s)' for 'words.' This lapse was corrected before 's' was completed; 's' was on the tip of the pen when writing was halted and the correction made...... This lapse is interesting in that it parallels another lapse recorded the same day, ('coats' for 'goats'). The word 'world' occurred in the preceding line, two inches to the left of the place where the lapse occurred."

3. "The auditory-vocal-motor cue was, clearly, 'give' but I wrote 'keep,' recognizing the lapse as soon as the word was written. Was unable to see any reason for such a lapse, then, recalling my observation of a month before, looked up and to the left—sure enough! there was the word 'Keeps' first word in second line above, one and five-eighths of an inch to the left."

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[1] This Journal, 1919, 30, pp. 274-290.

4. "Was taking notes on book I was reading. Wrote 'See Galton, p. 20g,' for 'See Galton, p. 209.' (Both G's were written in capital form but with a long down loop.) Distance from the 'G' in Galton to the interpolated 'G' was one and three-fourths inch. The graphic cue was largely visual, the number 209 as visualized on the page; no lip-motor consciousness. Possibly the lapse was due to grapho-motor perseveration, long down stroke on the 'g' (initial stroke on '9' and on 'g' being similar); possibly there was a visual suggestion operating from the peripherally seen G."

5. "Wrote on the margin of a theme I was correcting, 'Collected' for 'Connected.' Sentence read, 'Should be parallel in form and connected.' It was about two inches from the first 'l' in 'parallel' to 'l' as written in 'collected.'

6. "Was intending to write 'On the other hand;' I wrote, instead. 'On the who(le).' The auditory cue was 'hand' but perhaps the word 'whole' was there in the margin. Lapse was immediately recognized visually. After writing an account of the lapse I examined the writing tablet to see if a 'w' had occurred in the preceding line. The word 'what' was found in the line above, about one-half an inch to the right of where the lapse occurred."

7. "Wrote in a letter 'I've been hoping to see your article in print ere this. Of course it's hop(ing).' The verbal cue was definitely 'Of course it's worthwhile.' The lapse was recognized visually at once. It would seem induced by visual suggestion. In the manuscript the first 'hoping' is about two inches to the left and a half inch above the second 'hoping'."

Other sorts of lapses were of course recorded. Perhaps most interesting of all are those which result in a hybrid word, due to coalescence, of the intended and of the automatic word. Sometimes introspection reveals an inchoate or confused meaning-consciousness, which is highly instructive. One or two examples may be given.

8. "Wrote 'Emotent,' for 'Emotion constitutes the content.' This lapse was recognized as soon as written. (No analysis)."

9. "Wrote, 'Going to the door I saw Dig outstretched on the back porch.' There was a conflict in writing-cue between 'Nig' (name of our cat) and 'Dick' (name of our dog); 'Dick' prevailed as cue. It was not until rereading my notes next day that I perceived the coalescence of the two cues. This lapse escaped the visual censor."

10. "In correcting a student's theme I started to write 'please' for 'believe.' Recognized this error when I started on the below-line stroke of 'p' (a visual and motor recognition). Auditory-vocal-motor cue was a blurred 'b-l-l.' I had just been thinking out instructionss to class. I intended to say. 'Please save this theme.' There was a perseveration of meaning which was influential, together with the similarity in sound between 'b' and 'p,' in causing the lapse."

None of these lapses, except possibly 3 and 7 lend themselves to a Freudian interpretation. That Freudian lapses, motivated by unconscious complexes do occur I am convinced. I have, for example, written 'toxin' when I meant 'tocsin' and the like, but such lapses seem the exception, not the rule.

APPARATUS NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

Protractor for Color-Mixing. The annular brass protractor for color-mixing, which cost over six dollars before the war, costs considerably more now. Although it is very satisfactory in use, it is expensive when purchased in quantity for laboratory courses where several experiments in color-mixing go on simultaneously.

In the Clark Laboratory we have duplicated this protractor in a line engraving that can be printed on cardboard. The cut prints an annular ring, 13 cm. outside diam. and 10 cm. inside diam, with a scale of single degrees marked on both outside and inside circumferences. The center of the print is marked with concentric rings that aid in punching a hole for the disk-cutter. The outer circumference is cut entirely about by the disk-cutter. In cutting the inner circumference a bar (2 cm. wide) is left to stretch across the circle and to support in the center another annular ring that fits over the spindle of the mixer and centers the proctractor. The bar and central ring could be made of metal and the cardboard scale cemented thereto, but it seems to be satisfactory simply to cut the whole protractor out of the cardboard.

When printed on 4-ply board this protractor is stiff enough to stand ordinary usage, and thin enough to permit accurate reading at the edge. It is easy to read half-degrees on both inner and outer scales.

The Clark Laboratory will be glad to supply psychologists with these protractors, printed on cardboard but uncut, at cost which is at present about two cents apiece.

Artificial Daylight. Although the type C-2 Mazda lights do not give an exact artificial daylight, they provide a constant illumination that is more satisfactory when colored papers are used (demonstrations, the drill experiments in color, color preferences, *etc.*) than is uncontrolled daylight.[1] The uneven illumination can be avoided by having the lamps frosted at an electrical store. The frosting reduces the illumination scarcely at all; and it makes the lamp less uncomfortable in direct view.

E. G. B.

Exposure Apparatus for Memory Experiments. An inexpensive form of apparatus for successive exposures, devised by the late Professor J. W. Baird, has been found to be of service for presenting the materials of memory experiments in elementary laboratory classes and in group demonstrations. Nonsense syllables of black gummed letters, 1 in. high by 0.8 in. wide, are pasted on cardboards, 7 in. by 5½ in. These cards are then made into a booklet by the use of gummed cloth tape.[2] The tape is stuck on the bottom edges of the back of one card and of the front of the card behind it. In the same manner, all the cards are successively fastened into a booklet. A blank card is put at the front of each booklet. It is convenient in pasting the letters

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[1] Cf. G. J. Rich, this JOURNAL, XXX, 1919, 313-315.

[2] Cf. E. O. Finkenbinder, this JOURNAL, XXIV, 1913, 12.

on the cards to use a cardboard form with a rectangle of the extent to be covered by the letters of the nonsense syllable cut from the center. This form is placed over the card on which the letters are to be pasted, and assures uniformity in both the placement and the spacing of the letters. The cards are then notched or 'indexed' across the upper edge so that the experimenter can readily drop one page at a time. When the last card has been dropped, the booklet lies face downward on the table. The experimenter can then pick it up and give a second exposure of the series by again dropping the pages one at a time.

The booklet can also be made with a blank card at the end and the cards notched or 'indexed' on the side.[3] In this case the last blank page is attached by thumb tacks to an upright blackened board, in which a narrow slot of a size corresponding to the notching on the edge of the booklet is cut. This booklet is held up in position by a small metal hook which is passed through the slot in the board; and the exposures are made by moving the metal hook upward over the successive excisions in the cards. This form of booklet has an advantage over the first in that the observer by its use is less apt to be distracted by the sight of the hands of the experimenter S. W. F.

[3] Cf. S. C. Fisher, *Psychol. Mono.* xxi., (Whole No. 90), 1916, 35.

NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

By E. B. Titchener

1. Electromagnetic Control of Stop-Watch

This little instrument serves a twofold purpose: it safeguards the stop-watch against damage by careless handling, and it enables *E* or *O* to actuate the watch at a distance. The control was suggested to me by Jastrow's "Electric Attachment for the Stop Watch," which is figured in the catalogue of the Garden City Model Works, 1894, no. 11. It consists essentially of an electromagnet with pivoted armature, reinforced by a coiled spring. Watch and magnet are cased in a house of cast metal, with round window for the watch-face; and the clips that hold watch to stand, the pin that governs the tension of the spring, and the actuating button of the armature are all adjustable, so that the control may be applied to any make of watch. If the spring is set at the right degree of tension, and if the armature button is centred on the stem of the watch and moves in accurate alignment with the push-pin, the arrangement works—with the ordinary cheap stop-watch—on a single dry cell; a refractory watch may need two cells. The click at make, which is sometimes disturbing, may be lessened by papering the face of the armature.

The control is sold by the C. H. Stoelting Co., 3047 Carroll Avenue, Chicago, Ill.

2. Curve Tracer

It has always been difficult to draw accurately arcs of circles of large radius. Since these curves are variously useful in laboratory work, I have asked the C. H. Stoelting Co. to manufacture Weitzenböck's device (described by K. Bühler, *Die Gestaltwahrnehmungen*, i., 1913, 72). The model supplied to the Cornell Laboratory has been subjected to preliminary tests, and appears to work as accurately as the Wolz original.

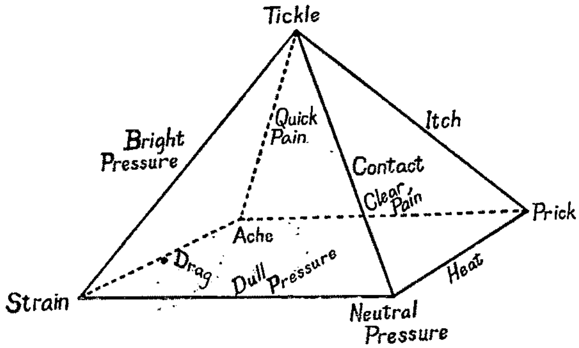
3. Models for the Demonstration of Sensory Qualities

There is nothing like a geometrical model for bringing home to beginners in psychology the nature of the qualitative series

and the positional character of the qualitative attributes. Some years ago I described a demonstrational color pyramid (this JOURNAL, xx., 1909, 15 ff.). I wish now to say a word regarding a set of four simpler (frame or outline) models that I have found useful for demonstration in introductory courses. Dimensions do not matter, though on general principles the larger the models are made, the better.

The frames are built of light metal rods, blackened, whose ends are held together by cubical blocks of blackened wood. The blocks are bored to receive the posts of paper flags, which show the initials (or other symbols) of the terminal qualities. The rods further carry one or more sliding blocks for the similar indication of intermediate qualities. The frames thus constructed will stand on any face; the flags may be interchanged for demonstrational purposes within the given model; and wires may be stretched as wanted across surface-planes and interiors.

The four models are the color double pyramid; the taste tetrahedron and the smell prism (H. Henning, *Zeits. f. Psych.*, lxxiii., 1915, 254 f.; lxxiv., 1916, 212); and a touch pyramid, here figured.



I need hardly say that several of my determinations of the touch qualities are tentative, and that I regard their arrangement at best as only a rough approximation to the truth. I publish the figure because I think it marks the direction of recent work; because it may perhaps serve as the starting-point of new experiments; and also for the more personal reason that I have shown the model in my lectures of the past two years, and that the observers in certain investigations shortly to be published from the Cornell Laboratory have therefore been familiar with its terms and series.

For the place assigned to heat, see F. Cutolo, this Journal, xxix., 1918, 445, 447 f.; for what I have called 'quick' pain, see E. Becher, *Arch. f. d. ges. Psych.*, xxxiv., 1915, 205. Warmth and cold for the present baffle me completely; I cannot bring them into qualitative relation with the terms of the figure; but the fact that they are left outstanding—I admit that it is a real difficulty—cannot of itself discredit the figure, and can still less discredit the underlying idea. Finally, auditory sensations may be systematized in various ways; and I have no doubt that some sort of closed figure will presently be found adequate to them. So long, however, as tonality and vocality are in debate, it is hardly safe to propose an arrangement of qualities.

4. Sewing Machine Motor

I am indebted to Dr. E. G. Boring, now of Clark University, for calling my attention to this motor, which is sold under the name "Sew E-Z" or "Home Motor," by the Hamilton Beach Manufacturing Company, Racine, Wis. The motor is intended for use with sewing machines, and in other household operations. For color mixing in the laboratory it has various merits; it requires practically no attention and stands hard usage. The motor is strongly cased, and its oil-cups are accessible. It will operate on either alternating or direct 110-volt current. It runs in both directions: the direction is reversed, very simply, by loosening a set-screw and shifting the brushes.

The motor is provided with a 6-speed rheostat, operated against a spring by a foot-treadle. The rheostat can be removed from its attachments, and turned by hand on the table. Thus separated, it is convenient and compact: 4 cm. high by 7 cm. diam. The motor itself with its base goes within a 15 cm. cube. The motor is light in weight, and though it is steady enough for ordinary color mixing must be clamped to the table if the discs are unbalanced (as in photometry by the Kirschmann method).

The motor has very little power, but great speed. Boring's tests show that on no-load the six points of the rheostat give about 2000, 4500, 7000, 9500, 12000 and 13000 r. p. m. A load of discs largely reduces this speed; but 20 cm. discs can be rotated on the fourth rheostat point at a speed of 4000 r. p. m., which is ample to prevent flicker.

The Cornell Laboratory has had the sewing machine motor in steady use since 1916, and the Clark Laboratory has recently adopted it. The current price (without discounts) of motor, rheostat, connecting cord and pulley is $18.50.

NOTE ON THE EXPERIMENTAL STUDY OF ATTENTION

By K. M. DALLENBACH

It seems necessary to reply briefly to two recent criticisms of my work on the experimental investigation of attention (this JOURNAL, xxiv., 1913, 465 ff.; xxvii., 1916, 443 ff.).

(1) With the remarks of K. Koffka (*Zeits. f. Psych.*, lxxiii., 1915, 281 f.) I am, if I understand them aright, in substantial agreement. But that, of course, is to say that I regard them as irrelevant, in so far as they are intended for a criticism of my method. Koffka, from the standpoint of his own theory of attention, attempts an analysis of the conditions of attention operative in my experiments. The conditions of observable clearness, however, mattered as little to my observers as would the conditions of the appearance of two greys in an experiment on the discrimination of brightness. We, by hypothesis, were observing a sensory attribute of the intensive sort, and were trying to correlate the quantitative changes of this attribute with objectively measurable variations. Clearness was the starting point, not the end-point, of the investigation.[1]

(2) H. Henning (*ibid.*, lxxxii., 1919, 245) adopts a more polemical tone, and is led thereby into several inaccuracies of statement. These are of minor importance: the serious thing is that he has failed to appreciate the significance, in this kind of work, of the 'wrong' judgments, and the interrelation of the Single Task and Double Task methods. He wholly misses the central fact of my results (though I naturally laid special stress upon it), that the curves of the 'right' judgments are skewed toward the higher, those of the 'wrong' judgments toward the lower degrees of observed clearness. A reader who should take Henning's review as adequate to my paper would, therefore, be misled.

[1] Britz' *Theoretische u. experimentelle Untersuchung ueber d. psychol. Begriff d. Klarheit* (1913) has been discussed by Titchener (*Psych. Rev.*, xxiv., 1917, 43 ff.). I have myself repeated and extended Britz' experiments, and give a critical account of the results in the *Journ. Exper. Psych.*

BOOK REVIEW

Anpassungstheorie des Empfindungsvorganges. By JULIUS PIKLER, Leipzig, Johann Ambrosius Barth, 1919.

1. *Hypothesenfreie Theorie der Gegenfarben.* 104 p.
2. *Theorie der Konsonanz und Dissonanz.* 34 p.

These two contributions to the study of visual and auditory theory clearly take root in a more comprehensive doctrine of sensory phenomena in general, which the author published in 1917 (*Sinnesphysiologische Untersuchungen,* Leipzig, 516 p.). It would seem that this latter doctrine has indelibly colored the University of Budapest professor's outlook upon data presented by the separate sense departments. The first part of the present monograph (published separately) treats of visual theory. Hering's views on this subject come in for the brunt of adverse criticism.

Bk and *W* are at opposite extremes of an uniform scale of brightnesses which passes through a central or normal point of middle grey. The somatic basis for the sensation of *Bk* is found in the complete absence of that process which conditions *W*, and the basis for greys in the varying degrees of partial absence of this process. The psychological antagonistic positivity of *Bk* derives from the representation in consciousness of a normal middle point in this process to which the organism has become *eingestellt,* and as a result of which deviations in either direction are perceived. The seeing of *Bk, W*, and greys, then, is conditioned by corrective adaptation processes with respect to the maintenance of an autonomous middle grey equilibrium. These adaptive corrections tend to raise or lower the point of equilibrium, from which in turn it follows that these same corrective processes in the direction of increase or decrease become greater or less,—whence the phenomena of simultaneous contrast. Positive after-images are due to the continuation of the adaptation process after cessation of stimulus; negative after-images to the excess of adaptation process after prolonged stimulation which is adequate to a new stimulus in the direction of greatest opposition to the previous sensation.

For the phenomena of color tone, Pikler elaborates and expands the above theory. In rough, his position is somewhat as follows: Colors distribute themselves along a scale in accordance with brightness values similar to the achromatic series. Each hue possesses a constant and "specific" tint. *B* is at the bottom of the brightness scale, *R* and *G* in the middle, and *Y* at the top. The following diagram represents the relation of the achromatic to the chromatic scale:

	Dark grey	Grey	Light grey	
Black				White
	Red blue	Red	Red yellow	
Blue				Yellow
	Green blue	Green	Green yellow	

The phenomena of color vision are explicable only in the light of their brightness components, since the somatic processes underlying

the latter are in reality the basis of the former as well. If this be true, then it is clear that *B* and *Y*, e. g., are antagonistic by virtue of their opposite location on the brightness scale. The same explanation given above for contrast, after-images, etc., is applicable to similar hue phenomena.

Part 2 is an extension of the author's general adaptation theory of sensation to the field of audition. Strangely enough, consonance and dissonance is the only subject treated!

The hearing of tonal intervals is fundamentally the perception of geometrical relations. We perceive in the octave the geometrical relation 1:2. The upper tone is the same as the lower, only doubled. This is explained in terms of the adaptation theory by the arousal of a sensation process by the lower tone of the octave, and a corrective adaptation sensation process just double in size by the higher tone. In the case of the fifth, we have the geometrical perception of the higher tone half double the lower, i. e., we perceive the ratio 1: 1+1/2, not 2:3. With the major third the upper tone is perceived to be the same as the lower, only one-fourth doubled, i. e., 1:1+1/4, instead of 4:5. These intervals are the only ones characterized by sameness of components, whence derives their consonance. All other intervals are perceived arithmetically, and the component tones are heard unlike and dissonant.

It is rather difficult to escape the conviction that facts of audition have here been forced into an hypothesis by dint of uncritical analogy with space perception. Remarks Pikler, "Der Gedanke, dass es auf dem Tonhoehengebiete geometrische Verhaeltniswahrnehmungen gibt, ist, soweit ich sehe, bisher niemandem gekommen." How stupid psychology has been!

C. C. PRATT.

BOOK NOTES

An Introduction to Social Psychology. By WILLIAM McDOUGALL. Lond., Methuen and Co., 1920. 459 p.

This is the fourteenth edition of this author's most important work. He has tried to deal with a difficult branch of psychology in a way to make it intelligible to the cultivated reader and without implying any previous familiarity with psychological treatises on his part. There is considerable elaboration in Chapter 3 of a principle formerly enunciated, namely, that all emotion is the affective aspect of instinctive process. He combats the current view that imitation is to be ascribed to an instinct, elaborates his conception of sentiment in a somewhat new way, analyzes the principal complex emotions in the light of the conception of the principle laid down in an earlier chapter on instinct and instinctive process, and finally applies the results of his theories to the description of the organization of the life of emotion and impulse.

The Problem of the Nervous Child. By Mrs. ELIDA EVANS. (With an Introduction by Dr.C. G. JUNG of Zurich.) N. Y., Dodd, Mead, 1920. 289 p.

After years of study of nervous children and parents by the psychoanalytic methods of Dr. Jung, the author here presents an account of the part played in the present life of the adult by mentally bad environment during childhood. The book has many practical illustrations of mistakes, and the cases showing how present nervous trouble in adults is directly traceable to childhood gives the work a certain interest for others than parents and teachers.

Lehrbuch der Logik. By TH. ZIEHEN. Bonn, A. Marcus and E. Weber, 1920. 845 p.

This comprehensive work is connected with the author's psychology and theory of knowledge but independent of them, as the work in pure logic should be. He deals first with the problem, then with the general theory of logic, with its relations to epistemology and psychology, the doctrine of judgment, and finally states his inferences and conclusions.

The Ground and Goal of Human Life. By CHARLES G. SHAW. N. Y., Univ. Press, 1919. 592 p.

The author here attempts a "treaty of peace" between the forces of individualism and those of social thought. In Book I, The Ground of Life in Nature, he deals with the self as thinker, the empirical ego, the surrender to naturalism; Part II of this first division discusses the struggle for the joy, worth, and truth of life. Book II treats of the Goal of Life in Society, including the transvaluation of self and society, the repudiation of sociality, etc. Book III, entitled The Higher Synthesis, takes up the question of the joy, worth, and truth of life in the world-whole.

Life Movements in Plants. By Sir J. C. BOSE. Calcutta, Bengal Govt. Press, 1919. 597 p.

This extremely able student of the physiology of plants here sums up his recent experiments, with considerable reference to those that have preceded. It is interesting to note that he has founded an Indian School of physiology, which is well subsidized and of which his country should be very proud. His ingenuity, originality, and independence of thought have enabled him to carry the subject of physiology of plants very distinctly beyond that which any of the many pioneers have attempted before.

Education in War and Peace. By STEWART PATON. N. Y., Paul B. Hoeber, 1920. 106 p.

Peace, no less than war, produces shell-shock, the symptoms of which interfere with individual efficiency, happiness, and social progress. This work calls attention to the urgent necessity of making adequate provision in our universities for training investigators competent to undertake the solution of the vital important educational problems now confronting civilization. The chapters are: Human Behavior in War and Peace, War and Education, and The Psychiatric Clinic and the Community.

A Beginner's History of Philosophy. By HERBERT ERNEST CUSHMAN. Bost., Houghton Mifflin, 1919. 407 p.

We have here Volume II, entitled "Modern Philosophy" (1453 to the present time). The author's work is intended as a textbook for sketch-courses in the history of philosophy and is written for the student rather than for the teacher. It is based on a background of geography and literary and political history, and its aim is to arrange and organize the material of the history of philosophy for the beginner. This volume begins with the causes of the decay of the civilization of the Middle Ages and ends with the philosophy of the nineteenth century.

Studies in Contemporary Metaphysics. By R. F. ALFRED HOERNLÉ. N. Y., Harcourt, Brace and Howe, 1920. 314 p.

These studies are described as "chips from a metaphysician's workshop" or "blocks hewn out experimentally in the effort after a systematic synthesis." They are like the painter's sketches or the sculptor's modelling in clay, which precede the finished work. The book consists of ten papers with a common animus but more or less independent of one another. He deals with the scientific method in philosophy, the philosophy of Nature, "doubting the reality of the world of sense," mechanism and vitalism, theories of mind, the self in self-consciousness, and religion and its philosophy.

The Intellectuals and the Wage Workers. By HERBERT ELLSWORTH CORY. N. Y., The Sunwise Turn, 1919. 273 p.

The chapters here are: Equality, Proletarianism, Religion, Criticism, History and Freedom, Liberty, The Class Struggle and Fraternity, and Education: A Program for the American University. It is dedicated to Carleton H. Parker, whose name suggests the spirit in which the author writes.

Spiritual Pluralism and Recent Philosophy.. By C. A. RICHARDSON. Cambridge, The Univ. Press, 1919. 335 p.

This work is dedicated to James Ward from the inspiration of whose teaching it derives its main theme. The author's purpose is to present and defend the theory that the texture of the Universe is through-and-through spiritual but he admits that he has been influenced by the Neo-Realists of America and by Bertrand Russell.

Pagan and Christian Creeds. By EDWARD CARPENTER. N. Y., Harcourt, Brace, and Howe, 1920. 319 p.

In this book the author of "Civilization: Its Cause and Cure" propounds a new theory concerning the origin of religious rites and ceremonies to prove that Pagan and Christian cults had the same source. He deals with the astronomical and magical aspects of the subject, the rites of initiation, redemption, sex-taboos, ritual dancing, legends of the Golden Age, the Eleusinian Mysteries, and the progress of Christian thought.

La Meccanica del Cervello. By LEONARDO BIANCHI. Rome, Bocca, 1920. 425 p.

This work sums up the extended investigations of the author on the mechanics of the cerebellum.

Aphasia and Associated Speech Problems. By MICHAEL OSNATO. N. Y., Paul B. Hoeber, 1920. 191 p.

This is a summary of the subject up to date.

COMMUNICATION—PSYCHOLOGICAL TERMINOLOGY

The Committee on Terminology of the American Psychological Association is taking up for examination terms in the fields of Sensation and Cognition. Psychologists interested in the precise use of terms are invited to assist the Committee in its work by calling the chairman's attention to—

(1) Psychological terms used with two or more different meanings (whether distinguished or not), and terms used indefinitely or ambiguously in contemporary writings.

(2) Pairs or groups of terms which lead to confusion when used interchangeably.

(3) Foreign terms needing definition or translation.

(4) Books and articles containing systematic lists of cognate terms, or discussions of ambiguous terms. (Full references desired.)

It is a matter of prime importance in any science to clear up double meanings and imperfect synonyms. The word *feeling* is used in standard psychological works with several different meanings. The words *intellect* and *intelligence* are used by some writers interchangeably, while others draw a sharp distinction between them. There are many instances in the literature of both kinds of confusion.

The Committee wishes to include a large number of such terms in its next report, either defining and distinguishing them or citing discussions in easily accessible sources. This list will not be confined to sensation and cognition, but will cover the entire field of psychology. Will readers of this magazine assist the Committee to make the list fairly complete?

HOWARD C. WARREN, Chairman,
Princeton University.

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AN EXPERIMENTAL STUDY OF VISUAL MOVEMENT AND THE PHI PHENOMENON[1]

By F. L. DIMMICK

TABLE OF CONTENTS

INTRODUCTION

In his "Experimentelle Studien über das Sehen von Bewegung"[2] Wertheimer reports the isolation of a new elementary mental experience, that of movement. To keep it distinct from the perceptual complex in which it usually occurs, he calls it *Phi* or the *Phi phenomenon.* Analysis of it reveals nothing for him but just the movement-experience; it has no visual quality; it has only spatial localization and direction.

The aim of our investigation is to put this elementary movement-experience, this Phi phenomenon, under critically descriptive conditions, and to test its analysability.

The term 'movement sensation' has been in common usage for a long time. Exner[3] first gave to it the meaning, in a pregnant sense,

[1] From the Psychological Laboratory of Cornell University.
[2] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych. u. Phys. d. Sinnes.,* 1 Abt. 61, 1912, 161.
[3] S. Exner, Ueber das Sehen von Bewegungen, *Wiener Sitz. Ber.,* Abt. 3, 72, 1875, 156.

of a primary and elemental experience. He cites experiments to demonstrate its immediacy, and points out its fulfillment of the characteristics of sensation. The peculiar nature of the movement experience was again emphasized by Schumann,[4] and an experiment to demonstrate its simplicity was indicated. Wertheimer followed closely the cue given by Schumann. He chose the simplest conditions under which the impression of movement could be obtained. Two lines, differing in position, were presented successively in the Schumann tachistoscope. The principal stimuli were a pair of parallel horizontal lines, and a pair of lines set at an angle to each other. A number of variations of these two stimuli were made in order to test the effect of different conditions. The *O*'s seem not to have been given specific instructions, but to have been allowed to report spontaneously what they experienced. They were, of course, kept in ignorance of the nature of the stimulus. Only three *O*'s were considered necessary, both because those with whom the work was started agreed well with one another, and because the reports were immediate.

The important determinant for movement, with the above stimuli, Wertheimer finds to be the temporal interval elapsing between the presentations of the two members. There is an optimal time, 60σ, at which the movement of a line all the way from the one position to the other results. With intervals either larger or smaller, the movement experienced is not complete. As a rule two objects are seen, both moving more or less, but not moving over the entire field. Such an experience we shall call bimembral movement. It sometimes happens that only one of the members seems to move, while the other remains stationary. This experience may be designated unimembral movement. A fourth type of movement is that in which there is no change of position of either member, but only a movement within a member itself, an intramembral movement. Under occasional or demonstrational conditions, Wertheimer is able to get the movement-experience alone, independent of any substantive perception and with no visual quality. Because it can thus exist in its own right, it is given the designation Phi or the Phi-phenomenon. It is this Phi, then, which, when added to the perception of a line in two successive positions, gives the impression of movement. It is the basis, too, of that seen movement which is correlated with a physically moving stimulus. The observations which he made led Wertheimer to outline a physiological theory of a short-circuit of nervous excitation in the cortex[5] as the basis of the Phi-phenomenon.

Experimental Procedure

For our experimental work, we used a modification of the Dodge tachistoscope which enabled us to present successively the two members of a bimembral stimulus. In addition it gave us control over the pre- and post-exposure fields.

The particular modification which we made of the Dodge instrument was the addition of a third stimulus-field by insertion of a second transparent mirror. A group of three plain-glass nitrogen-filled lamps (two 200 watt, and one 100 watt) furnished illumination for the stimulus-fields. They were placed at the ends of a set of three parallel tubes of about 5 cm. diameter. The light conducted down the tubes

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[4] F. Schumann, *Ber. ü. d. II Kong. f. exp. Psychol.*, 1907, 218.
[5] *Op. cit.*, 248.

was reflected from mirrors to the respective stimulus-fields. Between the lamps and the open ends of the tubes was placed a falling shutter, so designed that the exposures of the three fields were independently variable. The openings through which the light entered the tubes were horizontal slits, 1 cm. by 4 cm. To give additional control, an auxiliary shutter was set in every tube a little way from its open end, and with it a diaphragm which made possible the equation in sensible brightness of all three fields. The shutter fell from a constant height by its own weight when the current was broken through an electromagnet. The friction, as the falling shutter ran in the guides, was slight and the variation in time of fall proved negligible.

This arrangement made it possible for us to start with any pre-exposure field, to present the first member of the stimulus for a given length of time, then after a variable interval (during which the pre-exposure field was repeated) to present the second member for the desired length of time, and then to return to the pre-exposure field. The shutter permitted of total times up to 380σ; 210 σ was the longest needed. The eye-piece of the exposure apparatus was inside a dark chamber, and the *O* was allowed 10 min. to become partially dark-adapted before work was begun.

All of the stimuli described by Wertheimer were repeated, and six object-pictures of our own were added. The greater part of our work was done with two arrangements, the one of which gave movement from an oblique to a horizontal position, and the other from an upper horizontal to a lower horizontal position. There were four pre-exposure fields: A, uniform white, identical with the background of the stimulus-cards; B, similar to A, with a black fixation-point at the middle of the field; C, uniform lightless field; D, unlighted field, bounded at the top and bottom by luminous lines.

The exposure-time for each member of the stimulus was kept constant at 30σ. The time-intervals were 30σ, 60 σ, 90σ, 120σ, 150σ. They cover the range given by Wertheimer from near simultaneity to the region of succession. The principal stimuli, the angle and the horizontals, were presented in series of five exposures with every pre-exposure field, at every time, and to every *O*. Four other stimuli were presented to every *O* in the same sort of series, but with only two of the pre-exposure fields; they were so chosen that every stimulus was studied by one or two *O*'s and with two to four pre-exposure fields. A haphazard order of fields, times, and stimuli was followed in the presentation of the series.

Two different sets of instructions were given to the *O*'s, thus duplicating the above set of series. The one of these, which we shall call the 'process' instruction, was: "A stimulus will be shown you which will arouse a visual perception. Describe this perception in strictly psychological terms as accurately as you can. Report no process of which you are not sure. The stimulus will be repeated to complete the descrip-

tion." The other set, the 'meaning' instruction, ran: "A stimulus will be shown you which will arouse a visual perception. Characterize this perception as fully as you can. The stimulus will be repeated to complete the characterization." To three *O*'s the process-instructions were given first, and the entire group of series was repeated with the meaning-instructions. Three other *O*'s were required to give meaning-reports first, and were then shifted to the process-attitude. The two most practised *O*'s were instructed in successive series either for process or for meaning by chance, and every series was repeated later with the alternate instructions. Except for the last two, the *O*'s were required to read the instructions at the beginning of every observation hour. Before the regular work was begun, trial series were taken until the reports gave evidence of adequacy and stability.

The eight *O*'s who served were: Miss C. Comstock (C), graduate student in psychology; F. L. Dimmick (D), assistant in psychology; Dr. L. B. Hoisington (H), instructor in psychology; Miss M. F. Martin (M), graduate scholar in psychology; Miss A. H. Sullivan (S), graduate fellow in psychology; S. A. Takaki (Ta), graduate student in psychology; S. A. Tung (Tu), graduate student in psychology; and Mrs. A. K. Whitchurch (W), graduate student in psychology. Observers C, D, H, and S were practised; M, Ta, Tu, and W were relatively unpractised.

O sat with his head in the hood of the instrument. The auxiliary shutter was opened, a ready-signal given, the exposure shutter dropped, and the auxiliary shutter closed.[6] *O* then gave his report according to the instructions. The exposure shutter was raised to the starting position, and the stimulation and report repeated, until the series of five was completed. A rest of 2 or 3 minutes was permitted while the apparatus was reset for a new series. The above procedure was followed for the 160 series with every *O*.

Results

Table I summarizes quantitatively the results obtained with the two principal stimuli. Under our conditions, all *O*'s perceived optimal movement of the horizontal lines in more than 50% of the cases, and only one fell below that percentage with the oblique-to-horizontal movement. Of the perceptions

[6] The function of the auxiliary shutter was to shut off all light from passing down the tubes save when a true exposure was made. It eliminated the possibility of a false exposure when the falling shutter was raised to its starting position.

of partial movement, unimembral movement shows itself much more frequently than bimembral for all but two *O*'s. Simultaneity and succession occur but rarely. Closely paralleling the above perceptions are the percentages of reports, under the instructions to 'describe in psychological terms,' in which some visual process was present in the field between the two members of the stimulus. The number of times when the entire spatial interval was involved agrees with the number of optimal movements perceived; the frequency of cases in which the process in the interval accrues to one of the members and not to the other is approximately the same as that of the reports of unimembral movement; processes which involve both members but not the entire field occur as often as does bimembral movement.

TABLE I

PROCESS CONFIGURATIONS AND MOVEMENT PERCEPTIONS
(Percent. of 100 Reports for Every *O*)

Obs.	Horizontal Lines								Oblique Line and Horizontal							
	Total		Uni-		Bi-		None		Total		Uni-		Bi-		None	
	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M
C	78	77	20	15	3	8	0	0	48	35	43	58	8	6	1	1
D	66	61	22	21	12	16	0	2	77	73	12	14	11	14	0	0
H	66	56	24	24	3	15	7	5	67	66	25	19	5	10	3	5
M	72	65	23	21	1	11	4	3	100	88	0	12	0	0	0	0
S	77	69	6	7	13	23	4	1	90	72	1	23	1	7	8	0
Ta	63	52	25	28	10	17	2	3	60	65	30	33	7	2	3	0
Tu	77	69	22	21	2	9	0	1	80	69	20	29	0	1	0	1
W	60	53	21	22	19	25	0	0	68	77	19	15	13	8	0	0
Average	70	63	20	20	8	15	2	2	74	68	19	25	6	6	2	1
Mean Var.	6	7	4	4	6	5	2	1	13	10	11	11	4	4	2	1

Uni-, Bi-, = experiences of unimembral and of bimembral movement.
P = Process configuration; M = meaning characterization.

TABLE II

OPTIMAL MOVEMENT WITH DIFFERENT BACKGROUNDS
(Percent. of 25 Reports for Every *O*)

Obs.	Horizontal Lines								Oblique Line and Horizontal							
	A		B		C		D		A		B		C		D	
	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M
C	100	76	53	76	73	80	87	76	53	56	59	52	40	24	40	8
D	60	40	76	48	72	60	60	96	64	68	68	64	80	88	96	72
H	56	56	72	80	72	68	64	52	72	48	72	92	64	68	60	56
M	76	67	56	53	68	67	88	73	100	100	100	100	100	40	100	52
S	84	87	72	93	72	40	88	53	96	60	92	60	80	93	92	73
Ta	67	40	53	48	53	56	80	64	67	60	60	76	67	60	47	64
Tu	73	68	93	68	67	68	73	72	80	68	87	84	73	52	80	72
W	68	60	60	53	52	53	60	47	64	80	60	67	76	67	72	93
Average	73	62	67	65	66	62	75	67	78	68	75	74	73	62	73	62
M. V.	10	11	11	13	7	8	11	13	15	12	14	14	11	17	19	17

TABLE III

Optimal Movement with Different Temporal Intervals
(Percent. of 20 Reports for Every O)

	Horizontal Lines										Oblique Line and Horizontal									
Obs.	30		60		90		120		150		30		60		90		120		150	
	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M
C	67	55	92	95	92	90	83	80	58	65	25	0	62	50	75	70	50	35	25	20
D	50	75	90	65	80	85	80	45	30	35	70	45	95	90	95	95	70	70	45	65
H	55	50	80	65	80	75	65	60	50	30	50	65	90	95	90	90	55	75	50	30
M	50	75	75	92	90	75	75	75	70	8	100	83	100	100	100	100	100	100	100	58
S	60	83	90	92	95	92	95	42	45	33	100	92	85	100	95	92	100	42	70	33
Ta	50	70	83	55	92	65	42	35	50	35	50	50	83	60	92	100	50	75	25	40
Tu	75	70	100	95	100	85	50	55	58	40	75	75	83	90	100	80	90	60	50	40
W	15	25	70	75	90	83	75	42	50	42	60	67	70	92	100	100	80	67	30	58
Av.	53	64	85	79	90	81	71	54	51	36	66	72	84	85	93	91	74	66	49	43
M.V.	12	14	7	14	5	7	14	13	8	10	20	21	9	15	6	8	18	15	18	13

In Tables II and III the results have been arranged to indicate the influence of the pre- and intra-exposure field and of the temporal interval between members. Individual variations appear with the various fields, but they show no uniform tendency, and their nature is indicated in some cases in the introspections. C often objected that the fixation-point on pre-exposure field *B* interfered with her reports of process; S complained of the dark backgrounds. It is significant, however, that the process-reports and the perceptive characterizations parallel each other. This agreement is very evident when the averages for the group are taken, and it is further supported by the fact that the mean variations are not excessive. We may conclude, then, that the kind of pre-exposure field employed had no general and constant effect on the perception or its process-configuration.

Table III, on the other hand, brings out a very definite influence of the temporal interval. The *O*'s vary among themselves, but they agree in that they have curves of percentages which are high for the middle values of the temporal interval and low for the extreme values. Process-reports and perceptive characterizations run parallel. The averages, of course, show the conformity even more prettily. Time-interval 90σ has the highest average percent. of optimal movement and of total configurations; 30σ and 150σ have the lowest. Just as significant is the fact that the mean variations are smallest at the middle value. The larger variations among *O*'s occur with the longer or shorter times.

The evidence of the values, in the above tables, of the various perceptive characterizations indicates unquestionably that we have reproduced the essential factors of Wertheimer's conditions. Table I shows that our temporal intervals lie between those which give succession and simultaneity. The two forms of partial movement appear frequently, but optimal movement

predominates. The ineffectiveness of variations of the pre-exposure field does not contradict Wertheimer's observation that the color of the stimulus did not affect the perception. Finally, the behavior of the perception with the different time-intervals is exactly that which Wertheimer suggests, though he makes no precise statement.

On the other hand, in the column in which Wertheimer would write 'nothing,' under the heading 'process,' we have found a closely parallel set of values which represent the configurations of visual process present in the movement-field when the *O*'s were set to describe the experience. The nature of the visual experience which filled the interval can be pointed out most clearly by direct reference to the introspections.

Optimal Movement; Meaning Reports; Stimulus I

C. A horizontal line appeared in the upper part of the field, moved down to a lower position, and stopped; the movement was slow, smooth, and continuous.

D. I saw a horizontal line move quickly from a position above the fixation point to one below, and disappear. It seemed to come in, move down, and go out, as if that were only a part of some larger movement. The movement was rapid but smooth.

H. A black horizontal line appeared and moved down very rapidly, and then came to rest in a lower position for a short time. The movement was uniform but very fast.

M. A line moved from an upper horizontal position to a lower one; it slowed up just before it reached the final position; the movement was continuous all the way from one to the other.

S. A rectangle moved down smoothly and stopped for an instant. The speed of the line increased as it moved, and was very rapid just before it stopped.

Ta. A horizontal line appeared in the center of the field and moved down smoothly; it became shorter as it moved, and stopped in a position lower down.

Tu. A line dropped downward very smoothly and rapidly, until it reached the lower position, and then disappeared.

W. I saw a line drop smoothly and quickly from one position to another.

Stimulus II

C. I saw an oblique line which moved down across the field to the horizontal with a smooth and continuous movement.

D. A dark grey rectangle moved smoothly and rapidly from an oblique position to a horizontal.

H. An oblique line fell down into a horizontal position. The movement was continuous, but faster just before it reached the final position.

M. An oblique line moved down through an arc to the horizontal.

S. An object in the oblique position fell to the horizontal. The movement was rapid, and stopped short at the end.

Ta. An oblique line appeared, turned downward, and took the horizontal position. The movement was smooth.

Tu. I saw an oblique line first, and it moved steadily down into a horizontal position.

W. The oblique fell rapidly into the horizontal; I saw movement all the way.

PROCESS REPORTS; STIMULUS I

C. At the upper part was a black horizontal extent which immediately gave place to a grey patch below and joining it. The bottom of the grey was darker and at that place appeared later a black extent similar to the first. The grey changed in quality; it was one continuous extent, but it was different at different places.
D. A black line appeared just above the middle of the field. It disappeared, and extending down from where it had been was a grey band, light at the top and dark at the bottom, and at the lower side was a very dark strip of grey. The grey patch did not change in extent, but the patterning of brightnesses changed.
H. The upper line was not definite at the lower edge, but fused into a filmy grey that was a little darker at the right and left extremes, and terminated in a black process lower down.
M. There was a line of very dark grey at the top, and immediately below a darkening of the field in the general form of a rhombus, the edges of which were fuzzy but fairly definite. At the bottom of this figure was a definite line, and in the middle a darker streak which could hardly be called a line.
S. At the top was a black rectangle, clear and intense. Below was a grey patch, longer in the horizontal dimension, but not clear. At the bottom was a definite line.
Ta. At first an irregular rectangle, black and sharp at the top, appeared. Its lower side was grey and blurred. Below was a wide grey line, sharp at the bottom but indefinite above. The lower and upper sides of the upper and lower lines respectively shaded off and joined each other.
Tu. First, there was a line above the middle of the field; below it, a dark grey which faded out farther down until it reached a lower horizontal line, the upper part of which was grey and indefinite, the lower part black and sharp.
W. A horizontal line appeared and disappeared, and below where it had been there was a wide grey rectangle of light tint at the top, medium in the middle, and dark at the bottom. Below appeared a second line.

STIMULUS II

C. First there was a black oblique at the top, which immediately became greyer, and then was replaced by a grey patch, which extended down to the horizontal, where a line appeared.
D. The oblique came in first; then below it there was a grey which extended down to a horizontal line below; this grey was not uniform but light and dark in streaks.
H. An oblique line appeared and then a horizontal, and between them the field was grey in a solid fan-shape.
M. An oblique appeared; then below it there came a sector of pale grey which was darker near the bottom, where it became a more definite line. The first line had faded out before the last one came.
S. A black oblique line appeared; then something grey came below it which was rather indefinite, and extended down to the horizontal line.
Ta. I saw an oblique line which was blurred downward. The upper part was black and clear-cut. The lower part shaded off into a grey, at the bottom of which came a horizontal line.
Tu. I saw an oblique line and a horizontal, and the space between them was grey. The oblique was wide and had no definite outline on the lower side.

W. An oblique line appeared and disappeared, and then a horizontal came below. Between the two was a triangular light grey patch, which was darker near the bottom line.

UNIMEMBRAL MOVEMENT; MEANING REPORTS; STIMULUS I

C. A horizontal line moved down smoothly but not continuously. It moved part-way, then disappeared, then reappeared lower down and stationary.
D. The first line appeared and remained stationary. Below it another line came in, moving downward. The movement was over most of the space between the two positions, but was of the lower line.
H. A line appeared in the upper part of the field for an instant; then a little lower down something moved into the field, and stopped as a black line at the bottom.
M. A horizontal line came into the part of the field moving, then another appeared lower down and stationary.
S. A line moved down smoothly, getting faster as it went, and then the end-position stood out, sharp and stationary.
Ta. A line moved down, and as it moved another appeared lower down and remained stationary.
Tu. First I saw a line which wavered but did not change position, and at the same time there was something moving down into a position below.
W. A line dropped into a lower position. The motion was best near the bottom, and there was a blank space just below the top position.

STIMULUS II

C. An oblique and horizontal appeared, and the first moved down toward the second but never reached it.
D. I saw two lines, one oblique and one horizontal. The latter remained stationary, and the oblique moved down toward it, but was gone before reaching it.
H. An oblique line came in and moved downward, and below it stood a horizontal line.
M. An oblique line moved down toward the horizontal. The movement was jerky and definitely of the oblique, not of the horizontal.
S. I saw a line move down very rapidly through an arc. The first part of the movement was not clear, *i.e.*, it seemed to be part way down before I saw it; above was a blank.
Ta. An oblique line appeared, then a horizontal, and the oblique moved down to the horizontal.
Tu. An oblique line appeared in the upper part of the field and moved down faster and faster until I could not see the movement. Then the horizontal came in.
W. An oblique line moved rapidly to the horizontal. The movement was best at the beginning, and just at the bottom there was a space of no movement at all.

PROCESS REPORTS; STIMULUS I

C. I saw a black line; and below it was a grey band which was not uniform in quality, but which had no definite lines in it. Below was a second sharp black line.
D. Two lines came successively. The upper was clear-cut and stationary; the lower was at first a band of greys, the upper edge of which was indefinite and concave.

H. A black line appeared in the upper part of the field. Below and detached from it by a bright streak was a rather indefinite grey, which fused into a black line lower in the field.
M. First came a line in the upper part, then a grey, and another line below. The grey joined the lower line but was separated from the upper by a bright streak.
S. I saw a horizontal line, then lower down another with a grey patch above it.
Ta. The first line was wide; the upper part black, the lower part grey and blurred. There was a bit of white background between this and the lower line.
Tu. There was a sharp definite line. Just below it was white, and all the rest below was grey, darkening downward until it joined the lower line.
W. I saw two horizontal lines, one below and after the other. A band of greys extended up from the lower toward but not to the upper.

STIMULUS II

C. There was a wide patch of grey in the shape of a sector. It was dark at the top and shaded off into light grey downward. The grey did not reach to the horizontal line which came in at the bottom.
D. A sharply defined oblique line appeared and then a horizontal. When the horizontal came, the field just above it was greyish.
H. First there was an oblique line with a patch of bright field just below it; then at the bottom a narrow sector of light grey, which was later replaced by a horizontal.
S. A black oblique appeared first; below it was white. Then came a horizontal line, with an oblique grey on its upper edge.
Ta. The oblique was grey on its lower side and blurred downward. There was a white space between it and the horizontal line.
Tu. I saw an oblique line clearly; then the background below it became dark at the top and light lower down. At the bottom there was a horizontal line with a white streak above it.
W. A distinct oblique line appeared at the top, then a horizontal, and between them a series of grey triangles, light at the top and dark at the bottom and joined on to the lower line.

BIMEMBRAL MOVEMENT; MEANING REPORTS; STIMULUS I

C. A horizontal line moved down and stopped. The movement was not continuous; there was a place in the middle where it disappeared entirely. Farther down it came back again.
D. A horizontal line appeared and started to move, then disappeared, but reappeared lower down, and moved into the lower position.
H. A heavy black line appeared and started to move, but disappeared. Part way down it reappeared, and moved into the lower position and stopped.
M. Two black horizontal lines appeared and both moved down a little.
S. An object appeared and moved downward. A little way below another line appeared, and moved down and stopped.
Ta. A line appeared, moved down and disappeared; after a short space another appeared, and moved down and stopped.
Tu. A horizontal line moved down very rapidly, disappeared for an instant, then moved down again into the lower position. The upper part of the movement was the more rapid.
W. The line moved down quickly but with a flicker, *i.e.*, it started to move, there was a break, then the last part of the movement came out distinctly.

STIMULUS II

C. An oblique line moved down to the horizontal. The movement was not smooth; it disappeared in the middle so that there was a blank space.
D. An oblique appeared and began to move, but very soon disappeared. Then a horizontal appeared, which seemed just at the end of a movement down.
H. The oblique started moving, then for a bit I saw nothing, then the line moved from just above into its horizontal position.
S. An oblique line moved down to the horizontal, but there was a pause in the movement part-way down.
Ta. An oblique line came in and started to move, but soon faded out; then something below jumped in, and moved down to the horizontal.
Tu. An oblique line moved down to the horizontal but not smoothly; there was a flash of good movement at the beginning and end.
W. An oblique line dropped to the horizontal, but I did not see movement all the way. There was a blank in the middle.

PROCESS REPORTS; STIMULUS I

C. I saw a line in the field above the middle. Its edges were not sharp, and it shaded off at the lower side. Below appeared another line, which was surrounded with grey at first and then became quite distinct.
D. A horizontal line came in with a grey blur on the lower side. The grey extended farther down from the extremities of the line than from the middle, so that its lower side was concave though not sharply outlined. The second line had a similar grey above it. A roundish patch of white background was left between the two.
H. I saw a black line a little above the middle of the field, then another below the middle and equal in extent to the first. Between was a grey, with a slight gap of bright field in it.
M. There appeared two lines of grey. Both were indistinct on the edges that were toward each other.
S. I saw a line that was clear and black at the upper edge but indistinct below. Then came a space in which there was nothing, and then I saw a hazy line at the bottom.
Ta. The first line was grey all over and a little wider at the ends; the middle part had no definite outlines. The second line had black and clear outlines at the ends but the middle part was indefinite at the top, and the grey margin of it was a little concave.
Tu. First an upper and then a lower horizontal appeared, and the space between was grey except at the center, where it was the same as the background.
W. I saw a horizontal line, then another below it. Between them was a blur of grey, darker at the top and bottom and white in the middle.

STIMULUS II

C. I saw an oblique line. It disappeared and the field was only white, then a horizontal appeared. Both lines were indistinct and blurred toward the interior of the angle.
D. I saw an oblique line. It disappeared and I saw a horizontal. The oblique was at first a sharp line, then it shaded off into the background below it. The horizontal was indefinite along its upper edge when it came.

H. An oblique line appeared which was not sharply outlined. Just below it was a very light grey. Then a horizontal appeared, which was indefinite on its upper edge at first but soon became sharply outlined.
S. First there appeared an oblique line, which was not clear at the lower edge but shaded off into grey. Below there was an interval of white, and at the bottom a horizontal which also lacked definition.
Ta. The upper side and end of the oblique had clear-cut outlines, but the lower side was greyish and blurred down. The upper side of the horizontal had no definite outline.

For the demonstration of bimembral movement, Wertheimer recommends a stimulus consisting of two squares for the first member and a horizontal line for the second. This stimulus aroused bimembral movement in 94% of the times it was presented to our *O*'s and corresponding process-configurations in 84% of the times it was given under the process-instructions. The typical reports are as follows:

MEANING REPORT; SPECIAL STIMULUS

C. I saw a horizontal line and above it two squares. All three moved down a little and then disappeared.

PROCESS REPORT

C. Two squares appeared in the upper part of the field. They were grey and indefinite at the bottom. Below them came a black horizontal line, which shaded off into grey at the top.

The parallel values for the percentage of the three forms of movement are sufficient evidence that every perception of movement of these sorts has a corresponding process-configuration. The conclusion is further substantiated by the fact that in a number of reports the *O*'s have stated that a certain movement had a particular process-aspect and conversely that when, for instance, the grey was spread over the whole field, the movement was best.

Wertheimer's fourth movement phenomenon, intra-membral movement, appears according to his statement in the stage of simultaneity. Our *O*'s, likewise, reported a number of cases, when the temporal interval was shortest, in which the lines stood still, but in which there was a quick "jerking back and forth" or an "unsteadiness" as if the line moved inside itself. Under similar conditions, with the instruction to report process, the lines were sometimes of a multiform grey, variously patterned. Further analysis of intramembral movement, with the stimuli which Wertheimer says are especially favorable for its arousal, confirms what we have already said. Typical reports are as follows:

INTRAMEMBRAL MOVEMENT; MEANING REPORTS; STIMULUS III[7]

C. The line seemed to double over on itself at the middle; that is, there seemed to be a smooth gliding movement from left to right within the line which did not, however, change position.

W. I saw a horizontal line in which there seemed to be movement across, left to right. It was not movement from one position to another, for there was only one line and the movement was within it.

STIMULUS IV[8]

S. There was a square which contracted and expanded as if it were elastic.

Ta. A square appeared and stretched to right and left.

STIMULUS V[9]

M. An oblique line appeared and shrank towards its upper end until it became almost a square.

Tu. There appeared a line in the oblique position which seemed to move toward its upper end, but did not change its general position.

PROCESS REPORTS; STIMULUS III

C. I saw a horizontal line which was black at the right end, dark grey at the left, and light in the middle. The left-hand part was not evenly outlined, and there was no definite line where it joined the black part.

STIMULUS IV

S. First a black square appeared; then in its place was a greyish rectangle longer in the horizontal, then one darker and longer, then one black and slightly shorter.

Ta. I saw a black square which was fairly clearly outlined at first. Then it became greyish, and to the right of it was a horizontal line of dark grey. Then the whole figure was a horizontal line black at the left end and lighter toward the right.

STIMULUS V

M. An oblique line appeared. Then it was lighter at the lower end, and shaded into a dark grey upward. At the top was a sharp black square.

Tu. First I saw a dark grey oblique line; its upper end was darker, with a light place just below it. The lower part of the figure faded out into a light grey.

Movement within an object without change of position of that object thus presents nothing new. Under the characterization-attitude, it is movement on a par with the change of position of a form or object; descriptively, it is reflected in consciousness as the visual quality grey.

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[7] Stimulus III consisted of two horizontal lines, the second of which was in a line with but to the left of the first.

[8] Stimulus IV consisted of a square and a horizontal line of the same width. The centers of the two forms came at the same point.

[9] Stimulus V consisted of an oblique line and a square of the same width. The square came at the upper end of the position of the line.

THE PURE PHI PHENOMENON

Finally, a number of cases of Wertheimer's pure Phi were reported by our *O*'s. Frequently the line was not seen during the movement; there was simply "something moving" or just "movement." Sometimes neither of the lines was clearly perceived; just a bit of movement downward. The cases in which both lines were perceived with only movement between are entered in Table I as optimal movement, and are paralleled by total process-configurations. The appearance of movement detached from its object occurred, as Wertheimer notes, when the stimulus was new or for some other reason was incompletely perceived. Under exactly these same conditions, but with the instructions to report process, our *O*'s noted a flash of grey without the usual limiting lines above and below.

OBJECT-STIMULI

The remaining stimuli which were given by Wertheimer and which we repeated yield results agreeing with those already reported, but add nothing new to our analysis of the perception of movement. The six object-pictures which we added to Wertheimer's list showed the effect of central associations by giving optimal movement over the whole range of our conditions, and total configurations of process in all but a very few cases.

COLORED STIMULI

In the description of the stimuli used in his experiments, Wertheimer tells us that he varied the color of the members. In another place he remarks that partial movement results when the two members are not of the same color. Aside from this, the implication is that one color gives movement as well as another. After our main series had been completed, we presented stimuli of various colors to our *O*'s. The stimuli were shown in series of ten exposures at the optimal time (90σ) and with a pre-exposure field of the same quality as the background. The colors of these stimuli were red, green, and blue on white backgrounds, white and yellow on black backgrounds, red on a green background, and blue on a yellow background.

Movement, optimal in all but a few cases, was reported by all *O*'s for all colors and backgrounds when the instructions were to 'characterize.' Under the descriptive attitude there was always noted a *grey* patch covering the space between the two members. When the backgrounds were white or a light color, the grey was a little darker than medium. On the black field it appeared whitish. We may state, then, with assurance

that the grey which is the psychological equivalent of the perceived movement is under our conditions independent, for its quality, of the quality of the members of the stimulus, but shows an influence of the background which has the appearance of a brightness-contrast effect.

Korte's Reverse Movement

Korte,[10] in a later investigation, studied the perception of movement backward from the second member to the first, which, he said, resulted from making the second member more "intense" and by directing the attention to it. He varied "intensity" either by making the second member wider than the first, or by putting short perpendiculars across its ends. With an increase in "intensity" merely, only a slight backward jerk appeared. Neither was attention to the second line sufficient alone; but with an increase of "intensity" and a direction of the attention to the second member, the backward movement was plainly evident.

We attempted to repeat Korte's conditions. Our stimuli with members of different "intensities" were a grey line on white followed by a black line of the same width; a grey line on black followed by a white line of the same width; a black line of 1 mm. width on a white background followed by a black line of 4 mm. width; and a white line on black followed by a line of the same size with short perpendiculars across the ends. At first the *O*'s were given the usual meaning-instructions, and a series of ten exposures was presented. Then they were instructed to attend to the more "intense" line, and ten more exposures were made; and finally they were asked to try to get the line to move from the more "intense" position to the other. The perception was always from the member presented first to the second, save for a very few instances with the third instruction when *O* reported that perhaps there might be a hint of a jerk backwards; but even then no positive movement, such as they had seen with the other stimuli, was perceived.

We have repeated Korte's conditions as nearly as his description of them permits; and to his stimuli, which give spatial and form insistence, we have added intensive insistence. Since we never get a positive movement-perception as we do under Wertheimer's conditions, it seems evident that we are not in the presence of compulsory conditions, and that the two perceptions are not of the same order.[11]

The Nature of the Process-Configuration

Now that we have pointed out the quality of the psychological correlate of Wertheimer's Phi, we must emphasize that this is but one of its attributive aspects. It must not for a moment be assumed that it is a simple text-book grey, the grey that lies at the center of the color-pyramid, or the grey of a piece of grey paper. The temporal attribute is beaten up with

[10] A. Korte, Kinematoskopische Untersuchungen, *Zeit. f. Psych. u. Physiol. d. Sinnes.*, 72, 1915, 193.

[11] We are engaged in further experiments of this kind, the results of which will be reported later. We are also repeating certain experiments of F. Kenkel and V. Benussi.

the quality into an integration of the first order. All *O*'s describe the experience as a "flashing," "flickering," "unsteady," "shimmery," "flame-like," "liquid," "live" grey. It is further reported as a "curtain" or "film" which is not superficial but hardly bulky. The background seems at times to shine through the interstices of the grey, giving it a sort of transparency. In texture it resembles the *Flächenfarbe* described by Katz[12] and the adaptation-film of Sheppard.[13]

Conclusions

We have, in this investigation, been concerned primarily with a critically descriptive analysis of the reflection in consciousness of a stimulus-complex which is compulsory for the visual perception of movement. We have found that this reflection in consciousness takes the form of a primary integration of a visual quality (grey) with a duration which is characteristically brief. The integration is a multiform grey flash which is independent of the quality of the stimulus but is directly determined by the temporal interval between the exposures of the two stimulus-members. The spatial attribute is a constant factor for any one exposure, but may vary from exposure to exposure (as the perception of movement also varies between optimal and partial). There is, then, no movement in the multiform grey flash; the space is constant; the integration is of time and quality.

Wertheimer, finding that there "is no visual filling-in of the field of movement," argues that the perception of movement must have as its physiological correlate a short-circuit in the cortex. The grey flash which we have found to be the psychological correlate of the perception of movement obviates the necessity of such recourse to novel and speculative conditions.

[12] D. Katz, *Die Erscheinungsweisen der Farben,* 1911, 6ff.

[13] H. Sheppard, Foveal Adaption of Color, *Amer. Jour. Psy.,* xxxi 1920, 58.

A PSYCHOLOGICAL INTERPRETATION OF MODERN SOCIAL PROBLEMS AND OF CONTEMPORARY HISTORY: *A Survey of the Contributions of Gustave Le Bon to Social Psychology*

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1. GENERAL NATURE OF HIS METHOD AND PROCEDURE

Of the three chief psychological sociologists that France has produced—Tarde, Durkheim, and Le Bon, the latter is the most versatile, and yet by far the most superficial. In fact, the last may be regarded as a popularizer of the more striking ideas of the first two, especially of Tarde's views on imitation and Durkheim's notion of crowd-psychology. The range of his interests, however, is certainly remarkable. Trained originally as a physician, he gave up the practice of medicine, but has contributed several works on physiology and hygiene. Next he was employed by the French government as an archeologist and paleographer in the Orient. In recent years he has been editor of the *Bibliothéque de philosophie scientifique.* In addition to these activities he has occupied himself by producing a general work on social evolution in two volumes; studies of the chief historic civilizations; several contributions to mathematical chemistry and physics, among them a paper on intra-atomic energy which was published in a number of the leading scientific journals; a statistical study in physical anthropology; a work or two on education; and the some half-dozen books on social psychology which will form the basis of the present discussion.[1]

Of course it is obvious that a man who ranges at will over a dozen fields of research, any one of which could only be partially traversed with thoroughness in a lifetime, is not likely to have excelled in any of them. This is certainly true

[1] For a list of Le Bon's contributions see the article on Le Bon in *La Grande Encyclopédie* for his earlier works, and the biographical note in the *New International Encyclopedia* for a list of his main works. Another list of his works is given in the French biographical annual *Qui Êtes-Vous?* His productivity is perhaps only exceeded by that of his fellow-countryman, Solomon Reinach, who can hardly boast an equal breadth of interests. The writer has attempted to summarize the contributions of Tarde to this subject in an article in the *Philosophical Review* for May, 1919, and of Durkheim in the *Political Science Quarterly* for June, 1920.

of Le Bon, whatever his mental powers may be. Professor G. E. Vincent has thus characterized him in a fairly accurate manner: "M. Le Bon may be described as an intellectual kodak fiend. His books are filled with snapshots at truth, interesting in themselves, but sadly unconnected and out of focus."[2] At the same time, Le Bon's works are all highly interesting, and many of his generalizations sound plausible. His arguments are bolstered up by copious citations of a pertinent nature. Le Bon is one of those writers who exploits his theories in his own works. In discussing crowd psychology, for example, he tells the reader that the sure and certain method to be successful in convincing an audience of the truth of an assertion is to affirm the matter repeatedly, and, at the same time, to be careful to avoid any attempt at thorough analysis or any reference to a possible exception to its applicability. Nothing is more characteristic of Le Bon's own procedure than this very method. Taking a few rather striking psychological postulates which have the virtue of modernity, novelty, and suggestiveness, he applies these conceptions to nearly every phase of contemporary life in general and to French social conditions in particular. These theses are repeated and reiterated without detailed analysis or candid statement of exceptions to their application, until even a wary reader is likely to be beguiled by the facile phraseology of the author. Aside from his brilliant but uncritical dogmatism and "cock-sureness," another characteristic of Le Bon's socio-psychological writings should be noted. That is what Herbert Spencer would call his "anti-patriotic bias" and his "class bias." At least up to the outbreak of the World War, Le Bon could see little good in what he alleged to be the characteristics of the modern Romance peoples. Their assumed tendency towards a crowd-psychological condition and their desire to suppress individuality and put into power the incompetent masses reveal little of promise from his viewpoint. The oft-asserted Anglo-Saxon initiative, energy, will-power, and individualism, attract him as strongly as the alleged French traits repel him.[3]

Again, Le Bon finds little to arouse his enthusiasm in the traits of the masses; from his viewpoint progress and civilization are almost exclusively the contribution of the intellectually *elite*. There can be no doubt that Le Bon's exaggerations are in part due to his generalizations from French conditions,

[2] G. E. Vincent, in review of Le Bon's *The Psychology of Peoples*, *American Journal of Sociology*, January, 1899, p. 555.

[3] Probably Matthew Arnold would have found Le Bon rather comforting reading.

though even these he views in an extreme and exaggerated light. The relation of Le Bon's doctrines to his social environment is not of that subtle type which is likely to escape the attention of the reader, but is so prominent in all his works as to make them full of generalizations which are highly inaccurate and distorted when viewed as sociological propositions of general import. His fundamental doctrines—the idea of national character, the psychology of crowds and revolutions, his "anti-patriotic" and "class" bias, his continual scenting of impending calamities, and his bitter attacks upon socialism and syndicalism, are all directly and in large part traceable to his reactions to his French "milieu." At the same time, no one can deny that Le Bon has pointed out tendencies, conditions, and psychological laws which had previously been overlooked or undeveloped, and, when his works are read with the understanding which allows the discounting of his exaggerations and prejudices, they constitute an important contribution to sociological literature. It seems probable that Le Bon's contributions to social and political theory can best be understood through an examination of his main works on social psychology, noting their general doctrines briefly and devoting special attention to their bearing upon social and political problems.

2. The Psychology of Social Evolution

Le Bon's first considerable work in the field of social psychology was the volume entitled, *Lois psychologiques de l'evolution des peuples*.[4] This work purports to be a summary of the main psychological generalizations reached in his earlier works upon social evolution and the history of the civilizations of Asia, Africa, and Europe.[5] It consists mainly of what might be called psychological prolegomena to the study of history, though few historians or psychologists would agree to all of his generalizations. His main theme is the nature and importance of national character, or "the soul of a race," in the explanation of history and modern social problems.[6]

This all-important "racial soul" is the sum total of the moral and intellectual characteristics that lie at the foundation

[4] Paris, 1895, English translation, N. Y., 1898, reviewed by Professor Vincent, *Amer. Jour. Soc.*, January, 1899, pp. 554-6. Cf. also Bristol, *Social Adaptation*, pp. 133-138.

[5] *The Psychology of Peoples*, p. 230.

[6] Le Bon's notions of social evolution and of the contrasting characteristics of the French and Anglo-Saxon peoples are but a holdover of the doctrines of the "Romanticists," given a modern dress through a dash of psychology.

of the civilization of a race and determine the course of its evolution. The soul of the race finds objective expression in the totality of the type of civilization which distinguishes the particular race. "The moral and intellectual characteristics, whose association forms the soul of a people, represent the synthesis of its entire past, the inheritance of all its ancestors, the motives of its conduct."[7] In the formation of the racial soul the influence of the dead is preponderant. The racial soul is primarily unconscious; it underlies the rational phases of national thought and is, on that account, much more dominating in its influence. It is over this field of unconscious motives of conduct that the influence of the dead is particularly potent. "A people is guided far more by its dead than by its living members. It is by its dead, and by its dead alone, that a race is founded. Century after century our departed ancestors have fashioned our ideas and sentiments, and in consequence all the motives of our conduct."[8] These psychological characteristics which go to make up the soul of a race are composed of a relatively few fundamental ideas which are very permanent in character and are changed only very slowly, except through the effect of racial inter-mixture. Only the more superficial and secondary characteristics of a race are modifiable with any degree of rapidity.[9]

Le Bon contends that these races may be classified psychologically as well as anatomically. There are primitive races, or "those in which no trace of culture is met with," made up of peoples like the Fuegians; inferior races composed mainly of negroes; average races represented by the Mongolians; and superior races mainly exemplified by the Indo-European peoples.[10] The higher the race the more highly differentiated it is psychologically and the more superior minds it contains.[11] Though there may be a vast difference in the intelligence of

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[7] Op. cit., pp. 5-6, 63-64.

[8] Ibid., pp. 11, 15-16, 51ff. Le Bon admits that it is practically impossible to find a pure race at the present time in the sense of anatomical purity, and states that what he refers to are "historical races"—a product of psychological rather than physical evolution. An historic race is produced when two or more not too dissimilar peoples are brought together in fairly equal numbers and subjected to the same environmental conditions for a very long period of time. The apparent confusion which might arise from identifying the soul of a race with national character he explains by taking the ground that nations are normally subdivisions of some well-defined historical race and thus partake of the general characteristics of the race of which they form a part.

[9] Ibid., pp. 17ff, 154ff, 167ff.

[10] Ibid., pp. 25ff, Cf. Boas, *The Mind of Primitive Man*, Chap. I.

[11] *Psychology of Peoples*, pp. 39ff, 232.

the different individuals that go to make up a superior race, nevertheless the race is practically uniform in those fundamental psychological factors which determine its character.[12] This explains why it is that national character and not intelligence is the dominant factor in social evolution—why the English can hold in subjection the millions of India who equal or surpass the English in pure intelligence.[13] Even the most superior races cannot change the fundamental elements of their civilization with any facility. Cross-breeding of racial stocks is the only agency which will effect a rapid and fundamental change in national character. Social and physical environment have little strength as compared to heredity and inheritance.[14] " The history of civilizations is thus composed of slow adaptations, of slight successive transformations. If these latter appear to us to be sudden and considerable, it is because, as in geology, we suppress the intermediate phases and only consider the extreme phases."[15] According to Le Bon's view, therefore, history is nothing more than a product of racial character:

> History in its main lines may be regarded as the mere statement of the results engendered by the psychological constitution of races. It is determined by this constitution, just as the respiratory organs of fish are determined by their aquatic life. In the absence of a preliminary knowledge of the mental constitution of a people, its history appears a chaos of events governed by hazard. On the contrary, when we are acquainted with the soul of a people, its life is seen to be the regular and inevitable consequence of its psychological characteristics. In all the manifestations of the life of a people, we always find the unchangeable soul of the race weaving itself its own destiny.
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> The idea that institutions can remedy the defects of societies, that national progress is the consequence of the improvement of institutions and governments, and that social changes can be effected by decrees—this idea, I say, is still generally accepted. . . . The most continuous experience has been unsuccessful in shaking this grave delusion. . . . A nation does not choose its institutions at will any more than it chooses the color of its hair or its eyes. . . . Centuries are required to form a political system and centuries needed to change it. Institutions have no intrinsic virtue: in themselves they are neither good nor bad. Those which are good at any given moment for a given people may be harmful in the extreme for another nation. . . . To lose time in the manufacture of cut-and-dried constitutions is, in consequence, a puerile task, the useless labor of an ignorant rhetorician. . . . The conclusion to be drawn from what precedes is, that it is not in institutions that the means is to be sought of

[12] Ibid., p. 46.
[13] Ibid., pp. 33-34, 46-47.
[14] Ibid., pp. 53ff, 56ff, 81ff.
[15] Ibid., p. 96.

> profoundly influencing the genius of the masses. . . . Peoples are governed by their character, and all the institutions which are not intimately modelled on that character merely represent a borrowed garment, a transitory disguise.[16]

The soul of a race is very visibly and strikingly manifested in its political institutions. Applying this idea to French conditions, he finds that all the French parties, whatever their name, pursue the identical end of attempting to absorb the individual in the state and destroy individual initiative. In England and the United States, however, a different type of racial soul leads all parties to favor individual initiative at the expense of state-activity. All this goes to prove that, in reality, forms of government and political institutions in general count for very little in comparison to the psychological characteristics of a race. The great historical importance of the psychological characteristics of a race is well illustrated by the conspicuous success and expansion of Anglo-Saxon colonization and political forms in America and the equally apparent failure of the Spanish in this respect.[17]

Because of the very fact that cross-breeding is the only method by which it is possible rapidly to change the character of a nation, immigration on a large scale, with the consequent interbreeding, has a very important effect upon the destiny of a nation. Roman civilization perished more as a result of the peaceful amalgamation with barbarians than as a consequence of the subsequent military invasions. The same threatening conditions are now to be detected in the wholesale immigration into America, but thanks to Anglo-Saxon superiority the Americans may, if they act in time, exterminate these present barbarians as Marius did the Cimbri. If action is long delayed America must sooner or later meet the fate of the Roman Empire and disintegrate into many small and warring nations.[18]

Aside from the violent changes in national character which may result from wholesale racial intermixture, there may come about a more gradual modification, as a result of the infiltration of new ideas.[19] A new idea always arises in the

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16 Ibid., pp. 129-130; *The Crowd,* pp. 97-101. Cf. Ward, *Pure Sociology,* pp. 184-193; 544-575; *Applied Sociology,* pp. 13-17.

17 *The Psychology of Peoples,* pp. 130ff. "This terrible decadence of the Latin race, left to itself, compared with the prosperity of the English race in a neighboring country, is one of the most sombre, the saddest, and, at the same time, the most instructive experiences that can be cited in support of the psychological laws that I have enunciated." Ibid., p. 152. Le Bon's views are seriously compromised by the fact that he overlooks the historical elements in the situation.

18 Ibid., p. 154ff.

19 Ibid., p. 167ff.

mind of an individual who attracts a few enthusiastic disciples who aid him in zealously affirming its truth without analysis or discussion. But this soon leads to a wider and wider discussion of the merits of the idea by the public. If it gains ground it is spread by contagion and imitation throughout the society, and in time the group becomes as obsessed with the new idea as its originator was in the beginning. But, even with successful ideas, this is a very slow process. An idea never becomes a national obsession until, after years of discussion, it has filtered down into the unconscious strata of national character. When the idea has thus become a matter of dogma or sentiment it has reached its full degree of effectiveness.[20] On account of the sentimental and dogmatic nature of religious beliefs, which renders them especially amenable to fanatic support, ideas of this type have been the most powerful of all factors in the past history of mankind. To a large degree they have tended to shape the other types of beliefs and institutions.[21] In spite of the absurdities of past religious beliefs they have played an immense part in social control and in giving solidarity to society. There can be no doubt that the present tendency towards social dissolution is partially a result of the decay of the religious beliefs that society has outgrown, but which have not been supplanted by a new body of religious thought.[22]

Only by an application of social psychology can one comprehend the relation of leadership to social progress. While practically every real and substantial advance in culture is the result of the services of the *élite* in any society, they usually do little more than to synthesize the latent possibilities and tendencies of the age. Again, the truly *élite* never bring about any sudden or startling changes; they affect civilization only gradually. The great dramatic changes in history are the work of fanatics.[23] "At the bidding of a Peter the Hermit millions of men hurled themselves against the East; the words of an hallucinated enthusiast such as Mahomet created a force capable of triumphing over the Greco-Roman world; an obscure monk like Luther bathed Europe in blood. The voice of a Galileo or a Newton will never have the least echo among the masses. The inventors of genius hasten the march of civilization. The fanatics and the hallucinated create history."

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[20] Ibid., p. 169ff.
[21] Ibid., p. 190ff.
[22] Ibid., pp. 197-198.
[23] Ibid., p. 199ff.

As nations are built up by the formation of a national character, so they perish with its dissolution. As an organism decays when it no longer functions, so a nation disintegrates when it has lost its character. Le Bon finds at present many symptoms of decay among the Latin races of Europe, among which socialism, or the cult of state-worship, is the most menacing.[24]

Stated with their bold dogmatism and unobscured by being buried beneath a mass of erudition of another sort, these propositions of Le Bon sound rather novel and startling, but they are by no means new. His idea of national character as a vital reality, his belief in the superiority of certain races, and even his faith in the supremacy of the Anglo-Saxon or Germanic peoples is at the bottom identical with the doctrine preached in the works of Burke and the "Romanticists," of Freeman, Kemble, Green, and Stubbs in England and of Ranke, Waitz, Sybel, Droysen, and Treitschke in Germany. Further, his doctrine of the predominant importance of the ideas and beliefs of a people in their historic development is but an exaggerated statement of the conception of history as a socio-psychic process, stated by Lamprecht in Germany and now championed in America by Professor Robinson, and which seems likely to be one of the most fruitful lines of historical investigation for years to come.[25] This work on the "psychology of peoples" illustrates the main characteristic of Le Bon's sociological writings—an overworking of a very few important and perhaps not sufficiently emphasized principles of undoubted validity and significance.

3. The Psychology of the Crowd

Le Bon's second excursion into the field of social psychology was embodied in his most popular and well-known work, *La psychologie des foules*.[26] The ideas of this work, combined with the theories expressed in his *Psychology of Peoples*, constitute all the really important socio-psychological conceptions developed by Le Bon. His other and later works are but the reiteration of familiar doctrines and an application of them in greater detail to specific, historic, social, economic, or educational problems. Le Bon introduces the reader to his second work by a reiteration of the main thesis of the earlier

[24] Ibid., pp. 204, 211ff, 219ff.

[25] Lamprecht, *What is History?* chaps. i-ii; Cf. Robinson, *The New History*, chaps. i, iii, iv, viii. See also the article on "Psychology and History" in the *American Journal of Psychology*, October, 1919.

[26] Paris, 1895, translated *The Crowd;* London, 1896, reviewed by A. F. Bentley, *Amer. Jour. Soc.*, January, 1897, pp. 612-614.

book, namely, that the really significant historic changes are to be seen in the modification of human thought. "The only important changes whence the renewal of civilization results, affect ideas, conceptions, and beliefs. The memorable events of history are the visible effects of the invisible changes of human thought."[27] Few intelligent historians would disagree with this statement, however much they might dissent from some of Le Bon's exaggerated applications of the doctrine. The present era is a critical period, inasmuch as it is characterized by far-reaching transformations of human thought. The social, religious, and political beliefs upon which our civilization has rested are giving way before the growth of modern science and industry. The coming age seems destined to be the era of crowds, as a result of the growth of cities, the extension of the suffrage, and the improvement in communication. "The divine right of the masses is about to replace the divine right of kings." This prospect does not promise well for the future, for crowds are mainly given to violent action and are little adapted to producing careful and accurate thought. Civilizations have always been created by a small intellectual aristocracy, while the rule of crowds has ever characterized periods of decline and disintegration. There is no longer any hope of being able to overcome the rule of the masses. The popular movement has gone beyond the point where it might have been arrested. The only practicable method of meeting the inevitable tendency is for statesmen to acquire a knowledge of crowd psychology and thus be able to reduce the evil as much as possible through a scientific manipulation of the situation. Le Bon modestly suggests that it is the purpose of his treatise to make this much needed information available for the first time.[28]

In defining what he means by a crowd, Le Bon makes it clear that he does not regard a crowd as a mere group of individuals assembled in physical contiguity, but rather such an organized aggregation that a collective mind is formed and the conscious individuality of the assembled persons is practically lost. Not only may some aggregations fail to constitute a crowd, but on the other hand a whole nation may, with proper facilities for communication and a proper degree of psychic stimulation, assume all the essential characteristics of a crowd.[29]

Le Bon proceeds to enumerate the main psychic traits which, in general, characterize crowds. A crowd possesses a col-

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[27] *The Crowd*, 8th edition, London, 1913, pp. 13-14.
[28] Ibid., pp. 14-23.
[29] Ibid., pp. 25-27.

lective mind and a psychic unity which alters the normal emotion, thoughts, and conduct of the individual to a considerable degree. The crowd mind is not the average mind of its members, but is rather a complex of new traits which arise from the combination. The subconscious mind plays the predominant part in the psychic activity of crowds, and, as the subconscious is mainly charged with highly emotional qualities, with the archaic social inheritance of the race, and with the more common and instinctive content of the mind, these very qualities are brought to the front in the mental operations of crowds. In a crowd, therefore, the individual members are assimilated to a common mediocrity and the crowd is never capable of engaging in activities requiring a high degree of intellectual effort.[30] These new psychic traits which arise in the individual, as a result of his participation in a crowd, are brought about by several factors. In a crowd an individual feels a sense of invincible power quite absent in his normal isolated state. His susceptibility to suggestion is very greatly increased, and, as a result of this, the sentiments of a crowd are ultra-contagious. The net result of these factors is that in a crowd the individual behaves in a sort of half-conscious and hypnotic manner. "We see, then, that the disappearance of the conscious personality, the predominance of the unconscious personality, the turning by means of suggestion and contagion of feelings and ideas in an identical direction, the tendency to immediately transform the suggested ideas into acts; these we see, are the principal characteristics of the individual forming part of a crowd. He is no longer himself, but has become an automaton who has ceased to be guided by his will." At the same time, however, the action of a crowd under certain conditions may not be of an inferior sort. Owing to its tendency to act swiftly in response to a vigorous suggestion it may perform an heroic act if the suggestion it receives is of the type to promote such activity.[31] After thus analyzing the general psychic characteristics of crowds, Le Bon practically exhausts the list of qualifying adjectives in enumerating the special psychic traits of crowds. He finds that they are impulsive, mobile, irritable, suggestible, credulous, ingenuous, prone to exaggeration, intolerant, dictatorial, conservative, capable of entertaining contradictory ideas, of inferior reasoning powers, possessed of an abnormally sensitive imagination, religiously tenacious of a conviction, and likely to hold fundamental convictions with great firmness,

[30] Ibid., pp. 29-32.
[31] Ibid., pp. 33-38.

while exchanging superficial opinions with amazing alacrity.[32]

Le Bon's chapter on "The Leaders of Crowds and Their Means of Persuasion" is of interest as bearing upon his later discussion of the methods of modern political leaders. He finds that the leaders of crowds are almost uniformly rhetoricians or agitators obsessed by an idea, rather than careful thinkers, and that they tend to be very despotic in their methods of control. The successful leader gets the crowd to accept his belief by constant, dogmatic and repeated affirmation of his conviction without any attempt at reasoned analysis. Once an idea is accepted by a crowd it spreads with great rapidity by contagion and imitation. Leaders of crowds maintain their control by their prestige, which is either acquired by wealth or position or is a gift of nature. Napoleon possessed the quality of natural prestige to such a degree that it was sufficient to make an emperor out of his obscure and mediocre nephew nearly half a century after his glory had passed.[33]

Le Bon then proceeds to apply his theory of crowd leadership to an explanation of the method of persuasion of electoral crowds. In the first place, the candidate must possess sufficient prestige, as a result of ability, reputation, or wealth, so as to be able to force himself upon the electorate without any question or discussion of his lack of merit. Next, he should vigorously affirm, without attempting to prove, that his opponent is a scoundrel, having been guilty of several crimes. Then, he should flatter the electorate without any limit, making wide use of sonorous phrases condemning the wealthy and powerful and praising the virtues of the masses. While a candidate's written platform should be rather vague and moderate, he may make the most extravagant verbal promises, for the electorate always forgets them after the election. The voter forms no independent opinions, but has them forced upon him ready-made by the party leaders and orators. The guidance of the masses is a vital factor in modern civilization, for there is no longer any hope of destroying the doctrine of the sovereignty of the masses which has now become well-nigh a religious dogma. Even if it were possible to restrict the suffrage to the intellectual aristocracy there would be no reason to expect any decided improvement, since, by the laws of the psychology of peoples and crowds, assembled individuals tend to be ruled by their emotions and not by their intellectual faculties, and the emotional traits of the most erudite

[32] Ibid., pp. 38-88, 160-174.
[33] Ibid., pp. 133-159.

do not differ materially from those of the average individual. "In a crowd men always tend to the same level, and, on general questions, a vote recorded by forty academicians is no better than that of forty water-carriers."[34]

Parliamentary assemblies are another example of modern political phenomena which can only be explained by an application of the laws of social psychology. The whole system rests on the erroneous assumption that a large number of individuals are more likely to arrive at an accurate solution of a public problem than a small group. Parliamentary assemblies normally manifest most of the characteristics of a crowd. "The general characteristics of crowds are to be met with in parliamentary assemblies: intellectual simplicity, irritability, suggestibility, the exaggeration of the sentiments, and the preponderating influence of a few leaders." Perhaps the most significant special characteristic in their procedure is their almost invariable tendency to attempt to solve the complicated problems of public policy by a few simple formulas and by direct legislation. On questions of local or personal interest legislators have fixed and unalterable opinions. On general questions of policy and procedure, however, they are open to the suggestion of leaders, and if these leaders happen to be of about equal power but represent different opinions, the legislature will be conspicuous for its indecision and inconsistency, as it will vary in its response to the powerful sources of suggestion. The leaders enjoy their power as a result of their prestige or ability to arouse enthusiasm, and not on account of the logic or profundity of their arguments. Most great parliamentary leaders have been vigorous men possessed of the gift of florid oratory, but with relatively little breadth of mind or intellectual capacity.[35] While the action of a parliamentary assembly is normally on a little higher plane than that of the ordinary crowd, in times of excitement it degenerates into a mob, as in the case of the assemblies of the French Revolution. The sole salvation of parliamentary government lies in the fact that the laws are usually drafted by specialists and experts, and legislators merely vote for laws rather than frame them.[36] In spite of these shortcomings Le Bon concludes that, after all, parliamentary government is the best which has yet been devised. Its chief evils, in addition to the presence of the crowd psychological situation, are two—financial waste, resulting from the fear legislators have

[34] Ibid., pp. 202-212.
[35] Ibid., pp. 215-226.
[36] Ibid., pp. 226-231.

of opposing financial bills lest they lose their influence and their local appropriations, and the restriction of individual liberty, due to the worship of state-activity and the faith in the efficacy of the state to solve all social and economic problems. Le Bon finds that an excessive trust in the state is a symptom of national decline and thus ends this work in the same vein as the conclusion of the *Psychology of Peoples,* namely, that the Latin peoples who put their trust in the state are beginning their period of final decay.[37]

There is little that needs to be added in the way of comment on Le Bon's doctrines regarding the psychic traits of crowds. His general observations are in the main correct, but are highly colored and overworked. His treatment is doubtless inadequate, as he is guilty of the same faults that he finds in legislators, namely, of trying to solve a complex problem by a few simple phrases and formulas. As Professor C. H. Cooley very well says on this point:

> The psychology of crowds has been treated at length by Sighele, Le Bon, and other authors who, having made a specialty of the man in the throng, are perhaps somewhat inclined to exaggerate the degree in which he departs from ordinary personality. The crowd mind is not, as is sometimes said, a quite different thing from that of the individual (unless by the individual is meant the higher self), but is merely a collective mind of a low order which stimulates and unifies the cruder impulses of its members.[38]

4. Socialism in the Light of Modern Psychology

Le Bon applies the fundamental doctrines of the *Psychology of Peoples* and the *Crowd* to an interpretation of socialism in the third of his volumes on the psychology of modern social tendencies.[39] From the standpoint of social psychology there is very little indeed in this work which had not been suggested in the earlier volumes. The text opens in a strain strikingly similar to the doctrines expressed at the outset in *The Crowd.* A nation is controlled by a few fundamental ideas, the changes in which alone can effect any serious alteration in a civilization. Institutions are the effect and not the cause of the psychic traits of a nation. We are now in the midst of a

[37] Ibid., pp. 231-239. He strangely ignores the development of Socialism in Germany. The development of state-socialism in England since 1905 has been subsequent to the publication of Le Bon's work on crowd psychology.

[38] *Social Organization,* pp. 149-150. Le Bon's whole treatment of social psychology should be tempered by a reading of pages 61-205 of this work.

[39] *La psychologie du socialisme,* Paris, 1898; translated *The Psychology of Socialism,* N. Y., 1899, and reviewed at great length and with a large amount of psychological acumen by George H. Mead, *Amer. Jour. Soc.,* November, 1899, pp. 404-412.

critical period in history in which these basic ideas are in the process of modification.[40] Le Bon states that no one has yet analyzed socialism from the psychological standpoint and he volunteers to supply the lacking treatment. He finds plenty of causes for the recent growth of socialism: "The demoralization of the upper strata of society, the unequal and often very inequitable partition of wealth, the increasing irritation of the masses, requirements always greater than enjoyments, the waning of old hierarchies and old faiths—there are in all these circumstances plenty of reasons for discontent which go to justify the rapid extension of Socialism."[41]

Socialism, properly to be comprehended, must be viewed under four different headings; it is a political doctrine, an economic theory, a philosophic conception, and a belief. Its greatest strength, however, lies in its power as a belief.[42] Its importance as a belief arises from the fact that it appeals to the emotions rather than to reason, being in fact a new variety of religious appeal. It is a somewhat inferior type of religious ideal, however, for it does not appeal to the higher attributes of man's nature. Its power in this field grows out of the fact that it came at an opportune moment when the old religious beliefs were in a state of disintegration and before the new faith of the future had been developed.[43] But, aside from its primary appeal to the material elements of man's nature, socialism has a more fundamental weakness as a religious belief, in that it can give no promise of a future life and must make good its promises here on earth. This, however, it can never do, since its ideal is in fundamental conflict with those basic laws of psychology, economics, and political science which are beyond the control of man. Therefore, owing to the fact that it cannot realize its ideal, socialism will inevitably be discredited and begin to decline from the moment it has come into power in any country. The only good that socialism can effect as a religious belief is to act as a dissolving force which will clear away the wreck of the old religious beliefs and make way for the newer and sounder faiths that must characterize the future of humanity.[44] Reduced to their most fundamental terms all the varieties of socialism may be regarded as the cult of state-worship maintained by supporters of the obsession that civilizations are made and altered by institutions and governments. They all

[40] *The Psychology of Socialism*, pp. 1-2.
[41] Ibid., pp. vii-viii, 16.
[42] Ibid., pp. 3-4.
[43] Ibid. Introduction, pp. ix-xii, and pp. 85-103.
[44] Ibid., pp. 83-84.

advocate the doctrine of "collectivism"—collective ownership of wealth and collective control of industry, a complete absorption of all social activities by the state and the consequent suppression of all individual initiative and liberty, leading to the complete dictatorship of the state. In reality, the whole movement is in large part a futile protest against the inequalities of ability which have always existed and inevitably will continue to exist as long as humanity remains.[45]

All this Le Bon holds to be but the logical expression of the fundamental psychological traits of the Latin peoples, who, whatever their form of government, invariably favor the exaggerated rôle of the state and the suppression of individual liberty. They have not recovered from their old delusion that society can be revolutionized by decrees and constitutions.[46] The Latin mind has been prepared for this view for centuries and its present institutions are admirably designed to perpetuate it. This general attitude is favored by the Latin system of state education, with its mechanical drill in dead languages, its uniformity, and its lack of adaptability to individual variations and initiative; by its political institutions and doctrines which always laud the cult of state activity; and, finally, by its religion based on coercive and inflexible dogmas.[47] A proof of the assertion that socialism is essentially an expression of the Latin racial character is to be derived from the experience of the Anglo-Saxon and Germanic peoples in regard to socialism. In none of these countries has socialism assumed the revolutionary and doctrinaire form that it has maintained in the Latin countries. In Germany extreme socialism has gained a very slight foothold. It has there tended to become an opportunist movement under the form of social democracy or parliamentary socialism, in spite of the relatively ideal institutional adaptability of Germany to the socialistic regime.[48] In England and America socialism has not flourished among the native population because it is opposed to the national character, which favors individual liberty and private enterprise. In America, however, there is developing a great army of socialists composed of recruits "from the ever increasing flood of immigrants of foreign blood, without resources, without energy, and without adaptability to the conditions of existence in their new country." The only solution Le Bon can see for this difficult problem in

[45] Ibid., pp. 27-30.
[46] Ibid., pp. 33-36, and Book III, particularly pp. 140-148.
[47] Ibid., pp. 140-166.
[48] Ibid., pp. 104-110. In this generalization of Le Bon there are serious historical errors in matters of detail.

America is for the native Americans to arise and exterminate this undesirable multitude.[49]

Taken apart from all specific aspects socialism may be regarded in its most fundamental and general sense as the attempt on the part of the unadapted in modern civilization—"the unutilizable superfluity"—to make a place for themselves, and, if successful, to seize for themselves all the fruits of our modern civilized life to which they have not contributed a whit. The most dangerous aspect of the situation is the fact that we not only have at present a vast number of these unadapted individuals, but that our society is so constituted that it is increasing this number at an alarming rate. It will be the main social problem of the future, to which task socialism is obviously unequal, to care for these unadapted.[50]

Le Bon thus arrives at the conclusion that the advent of the socialistic régime would not bring the millenium in its wake, but its result "will be hell, a terrible hell!"[51] Nevertheless, socialism must be tried out in some country in order to convince the modern world of its absurdities. At the same time, it is the duty of every patriotic citizen to prevent the experiment from taking place in his own country. The *élite* in modern society must oppose the leaders of the socialistic movement by making use of the fundamental principles of crowd leadership—affirmation, repetition, contagion, and prestige. The task would not be difficult if there were sufficient desire and will power manifested by able citizens. Especially urgent is the necessity for the reform of the Latin system of education. If the *élite* do not rally to their duty the present inhabitants of the Latin countries may make proper preparations "to give place to more vigorous peoples, and disappear from the face of the earth."[52]

Le Bon's treatment of socialism, like his analysis of national and crowd psychology, suffers from an oversimplification of the factors involved from serious indifference to historical facts, and from an exaggeration of many of the undoubted defects which exist in the socialistic program. Many of his criticisms of the psychology of the movement rest upon a substantial amount of truth, but his account of the historic factors involved in the origin of socialism is lamentably weak,

[49] Ibid., pp. 111-125. Perhaps Le Bon will regard the present attempt to deport members of the Communist Party in America as a diluted substitute for his solution of the problem of socialism in this country.

[50] Ibid., pp. 358-383.

[51] Ibid., p. 406.

[52] Ibid., pp. 411-414.

and the economic, political, and philosophic analysis suffers from the excessive emphasis upon the psychological factors and from his extremely evident bias against state-activity and proletarian democracy. Le Bon's defense of individualism and his attack upon state-socialism in all of his works constitute the most frantic and dogmatic psychological defense of *laissez-faire* which has yet appeared.

5. The Psychology of Politics and of French Political Tendencies

After abandoning the field of social psychology for a decade, during which time he devoted himself to a study of physics and produced his volume on *The Evolution of Matter,* Le Bon resumed his earlier studies in his *La Psychologie politique et la défense sociale.*[53] This is essentially a study of the general tendencies of modern French political life, and hence has a limited bearing upon the general problems of political psychology. Indeed, it is unreliable even as a study of French conditions unless the reader is aware of Le Bon's decided "anti-patriotic" and "class" bias, which was clearly in evidence in his earlier works. Again, it should be borne in mind that Le Bon is constantly on the alert for instances which will confirm his preliminary assumptions and does not confine himself to a purely objective study of actual tendencies and conditions. There is a distressingly small amount of new theoretical material in this volume, and Le Bon excuses his repetition of his earlier dogmas on the ground that repetiton is the most powerful agent to produce conviction as to the truth of an argument.[54] Le Bon justifies the need of such a work as he had written by stating that while a knowledge of political psychology is absolutely essential to a successful statesman, the present available information on this subject exists in nothing but a few over-simplified formulae which are derived from experience and tradition.[55] He asserts that Machiavelli's *Prince* is the only real treatise on political psychology that has yet appeared, and, as this is somewhat out of date, he generously applies himself the task of supplying this serious gap in human knowledge.[56]

[53] Paris, 1910, reviewed by Prof. Vincent, in the *Amer. Jour. Soc.,* Sept., 1910, pp. 267-269.

[54] Op. cit., pp. 11-12.

[55] Ibid., p. 5.

[56] This naive oversight or omission, ignoring as it does the previous and much more valuable works of such writers as Bagehot, Tarde, Durkheim, Ratzenhofer, Simmel, Ross and Giddings in the field of political psychology, is not a high testimonial to the knowledge of the literature of the subject possessed by either Le Bon or his adviser, Ribot.

The work opens with a reiteration of the familiar dogmas that civilization is the product of national character and not of institutions; that it is futile to attempt to remodel society by transforming institutions; that government has great powers for destruction but very feeble ability in constructive work; that the place of reason in the psychic traits of society is very small; and, finally, that in France there is in reality only one political party, the sole aim of which is to increase the scope of state activity. Aside from these premises that are basic in all his works, Le Bon expands another conception which was suggested in his *Psychology of Socialism,* to the effect that the former rule of kings and laws is now being replaced by the domination of economic forces.[57] This doctrine is important in his theorizing, for he finds that most of the dangerous tendencies in modern political life, both despotic and anarchical, are the result of economic causes operating upon or through national character.

In discussing the subject of the psychology of laws and the effect of ill-advised legislation, Le Bon points out the fact that all real and valuable laws are nothing but the codification of customs previously recognized in judicial action. The rôle of law-making should, then, be confined to the codification of persistent and well-established customs.[58] The legislator who attempts to change the fundamental trends in social evolution does not differ fundamentally from Xerxes who whipped the sea as a punishment for the loss of his ships. These ill-advised laws, passed under the obsession that state-activity can remedy all social evils and maladjustments, not only fail to accomplish the intended results, but also create new evils which are often more menacing than those which the laws sought to correct.[59]

In their fundamental reality, political activities have never been anything except a struggle between phantoms. History looked at from a broad viewpoint appears as a continuous effort on the part of the people to create and destroy phantoms. These phantoms are of three main classes. The most powerful type is to be seen in the great religious, philosophical, and political beliefs of history; the intermediate type is represented by the myths which grow up about alleged heroes like Ulysses, Alexander, Caesar, Charlemagne, Barbarossa, and Napoleon; and the lowest variety is manifested by the petty

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[57] Ibid., p. 15.

[58] " La coutume résulte des nécessités sociales, industrielles, économiques de chaque jour. La jurisprudence les fixe. La loi les sanctionne." Ibid., p. 45.

[59] Ibid., pp. 50ff.

and transient phantoms that guide the ordinary beliefs and daily activities of the masses.[60] These phantoms gain power in proportion to the fear they create. The growing power of the working-classes has made them feared by the government, and, as a result, the government of France for twenty years has been solely in the interest of this class. Among the Latin peoples one of the most important transformations of phantoms in recent years has been that of the substitution of the phantom of the divine right of the state for the older phantom of the divine right of kings. This has made the present what may be called the era of functionaries.[61]

Le Bon applies his psychological analysis to what he regards as some of the more obvious present-day political "phantoms." He believes that the modern agitation for the cessation of war is a threatening sign. Wars are apparently not on the decrease and civilization seems powerless to remove their causes. Therefore, they may be regarded as necessities imposed by nature to secure national vigor and discipline.[62] The important services of wars are overlooked by the "sentimental pacifists," since, in reality, wars are the main forces which create and give stability to national character, and they are the most powerful of stimulants in developing the moral life and industrial activities of a nation. The detrimental effects of an enforced peace are to be seen in the cultural stagnation and the famines in India during the period of artificial peace imposed by the British régime. Thus the ill-advised humanitarianism of pacifists is really a menace to the public welfare. In the place of political wars, there is an equally deadly, though bloodless contest going on in the economic strife between nations.[63]

Again, the French system of education with its mechanical uniformity in the technique of instruction, its antiquated subject-matter, and the tyrannical state control of university instruction is contributing to the intellectual decay of France, when in reality it should be the fountain head of its intellectual life. While it is true, as Leibnitz maintained, that a proper system of education is able to transform a people in a century, it is equally true that a poorly designed and deadening system

[60] Ibid., pp. 61-62.

[61] "L'Etatisme a pour expression et soutien le fonctionnarisme." Ibid., pp. 69ff.

[62] Ibid., pp. 84ff.

[63] Ibid., pp. 91-102. This section of Le Bon's work should be compared with Nicolai's *Biology of War;* see also his own revised post-war views, section 9 below.

can bring about national degeneration in an equal period.[64]

Le Bon next proceeds to investigate the question as to whether there is any scientific basis for the modern extension of political power among the masses. He decides, in agreement with Faguet, that this tendency is in direct opposition to the dictates of all intelligence and experience. Modern scientific and political development has tended to accentuate the differences in mentality between the *élite* and the masses. The refined methods of modern science and the exacting requirements of the conduct of modern large-scale industry call for the highest type of minds. On the other hand, the introduction of machinery into industry and the great extension of the principle of the division of labor have tended to reduce very greatly the necessity of high mentality on the part of the laborer. Yet, in spite of this, the tendency in political theory and practice of late has been towards giving greater political authority to the masses whose mental powers are continually on the decline.[65] Fortunately, however, the masses themselves have little initiative and their action depends upon the nature of the leadership which they receive. It is evident, thus, that to save society the *élite* must assume control of the leadership of the populace and check the evil influences of demagogues and revolutionists.[66] Prestige, affirmation, repetition, and contagion are the fundamental principles to be employed in the art of persuading the multitude, and the *élite* must make use of them if they hope to wrest the control of the masses from the dangerous leaders who now direct them.[67]

Parliamentary government has even become unpopular in France. This has come about as a result of the indiscretion of the different parties in trying to outbid each other in promises to the masses in the hope of being successful in elections. Naturally they have been unable to fulfill these exaggerated promises, even by the most arbitrary and debasing use of the law-making power. As a result, they have stirred up bitter

[64] Ibid., pp. 103-117. Cf. Le Bon, *La Psychologie de l'education*, Paris, 1904. This view seems to contradict his theory of the stability of national character.

[65] "Tandis que les progrès scientifiques amenaient les élites de mentalité supérieure à diriger le mécanisme de la vie moderne, les progrès des idées politiques conféraient de plus en plus à des foules de mentalité inférieure le droit de gouverner et de se levrer par l'intermédiaire de leurs représentants aux plus dangereuses fantaisies." *La Psychologie politique*, pp. 118ff.

[66] "C'est donc aux élites à s' adapter au gouvernement populaire et à endiguer et canaliser les fantaisies du nombre, comme l'ingénieur endigue et canalise la force d'un torrent." Ibid., p. 122.

[67] Ibid., p. 136. Pages 118-141 are mainly a summary of his previous doctrines regarding the psychology and leadership of crowds.

animosity against the very system of government they represent.[68]

The mind of the working-class is essentially identical with the conventional mind of the crowd, but to these general characteristics are added the possession of certain special dogmas which are the result of their peculiar history, position and interests. The most prominent of these accessory beliefs in the mind of the workingman is the notion that wealth is created by labor and appropriated by the rich, and the consequent deduction that justice requires that the present class constitution and economic organization of society shall be overthrown and the laboring classes installed in their rightful position.[69] A new and particularly menacing development in the aspiration of the masses is to be found in the pretensions of syndicalism, which proposes to substitute for loyalty to and action by the state, loyalty to a particular industrial profession, and autonomy and self-government for this profession. They desire to substitute *l'égoïsme corporatif* for *l'intérêt générale du pays.*[70]

Socialism and syndicalism are the two most dangerous tendencies in modern political life which have developed out of proletarian activity, and the French government, pressed by the pretensions and demands of these two movements, has tended steadily towards a popular despotism.[71] Le Bon examines, criticizes and contrasts what he calls the " illusions " of socialism and syndicalism. His treatment of socialism is essentially a brief repetition of the doctrines advanced in his *Psychology of Socialism* and need not be repeated here. Syndicalism is one manifestation of the general modern tendency towards the association of similar interests. While essentially a revival of the ideas of the medieval guild, it has received its vital impulse from the new conditions in industry that have grown out of the Industrial Revolution.[72] Though both socialism and syndicalism are a menace to civilization they are fundamentally opposed in principle. The former would augment the rôle of the state until its activities become all-embracing, while the latter would divide society into many au-

[68] Ibid., pp. 163-175. Le Bon seems serenely unconscious of the direct contradiction between this view and the statement in *The Crowd* that a political leader could safely make the most extravagant promises and successfully rely upon the electorate to forget them after the election.

[69] Ibid., pp. 142-153.

[70] Ibid., pp. 153-162. Durkheim is much more sympathetic with certain phrases of syndicalist doctrine.

[71] Ibid., pp. 176-186.

[72] Ibid., pp. 202ff.

tonomous professional groups and dispense with the state, thus tending more towards anarchy than towards political absolutism. This fundamental divergence between the two movements, and the struggle which will be inevitable, may have the beneficial effect of preventing an extreme development of either tendency, but there will be the accompanying danger that society will be crushed between the opposing forces.[73]

Le Bon next turns to a long discussion of the psychological blunders involved in the French colonial system and procedure,[74] and then analyzes the various forces in French society leading to social anarchy, enumerating the main symptoms which are indicative of such a tendency. He finds that there is a general trend towards social anarchy and an accompanying anarchic mentality; that there is an increase of crime and a spread of criminal tendencies; that the habit of assassinating rulers and statesmen is becoming common; that the persecution of religious orders is popular; that there is a dangerous enmity and struggle between the different classes in society; and finally that there is a threatening fatalistic attitude, even among the most learned, which inclines them to regard human powers as unable to conquer the tendencies in external events.[75]

Le Bon devotes the last section of his work to a discussion of the chief steps which should be taken by society to save itself from the disintegration which threatens it as a result of the many anarchical and revolutionary tendencies that he has noted, in other words to an analysis of the "défense sociale." When a national mind disintegrates the people tend to revert in mental traits to the state of intellectual barbarism from which they were raised by the formation of a national character. This return to barbarism is being witnessed in France today. Society remains apathetic in spite of the assaults upon its integrity. As a matter of fact, however, the leaders in the defense of society need more enthusiasm than the leaders of the masses who are assaulting the foundations of society, for it is harder to convince people of truth than to get them to accept error.[76] But civilization cannot be maintained without

[73] Ibid., pp. 202-225.

[74] Ibid., pp. 226-284.

[75] Ibid., pp. 285-357. Le Bon evidently overlooks the fact that his own doctrine that events are the inevitable expression of national character which cannot be changed by legislation is of a decidedly fatalistic cast. He also seems unconscious of the fact that there is a contradiction between this position and his frequent assertion that there is a most powerful tendency in the direction of extensive state-activity.

[76] "L'erreur passionne, les froides vérités n'enthousiasment pas." Ibid., pp. 360-362.

effort. The apathy of the French *bourgeoisie,* who must bear the brunt of the social defense, is an appalling contrast with the ardor of the revolutionary element. Besides renewing its vigor and enthusiasm the *bourgeoisie* must give up the ostentatious and insolent luxury which is the most potent force in stirring up the animosity of the working classes. The only real evidence of a rallying of the *bourgeoisie* to the defense of society is to be seen among its more humble members, such as the small shopkeepers who have banded together to carry on a collective and cooperative struggle.[77] To be effective the social defense must not only have able and enthusiastic leaders, but also some fundamental doctrines to guide them. The corner-stone of this program should be the ideal of national defense.[78] To this must be added the consideration "qu'un peuple ne peut vivre sans armée, sans hiérarchie, sans respect de l'autorité, sans discipline mentale." These ideals would suffice, but they must be supported by the *élite* who at present manifest an alarming degree of apathy, cynicism, and fatalism.[79]

Le Bon's somewhat questionable premises and his "anti-patriotic" and "class" bias, which tend to make his conclusions rather suspicious, have already been referred to, and the detailed refutation of his specific charges against French society could be accomplished only by a detailed exposition of actual conditions. It would seem that his picture is scarcely accurate in general outline, and is even more highly distorted in matters of detail.[80]

6. The Psychology of Opinions and Beliefs

Le Bon's next venture in his system of social psychology is entitled, *Les Opinions et les croyances, genèse, évolution.*[81] From every logical standpoint this work should have been the starting point of his system, for it consists of an elaborate psychological defense of the main theses which have been the guiding principles in all his works. It seems that Le Bon has pursued the rather vicious circle of starting with some

[77] Ibid., pp. 363-366.

[78] "L'amour de la patrie forme le véritable ciment social capable de maintenir la puissance d'un peuple." Ibid., p. 370.

[79] Ibid., pp. 371-372.

[80] Several short works tending to modify Le Bon's view of French society are Brownell's *French Traits;* Wendell's *France of Today;* Guérard's *French Civilization in the Nineteenth Century;* Dimnet's *France Herself Again;* and Bracq's *France under the Third Republic.*

[81] Paris, 1911.

preconceived notions of social psychology,[82] using these to develop a system of social psychology, and then ending by employing the works built upon these ideas to substantiate the psychological analysis. The main thesis of this work is his familiar doctrine that opinions and beliefs have an affective and mystic, rather than a rational, origin and foundation. After dealing with the psychology of opinions and beliefs in the mind of the individual, he turns in books six to eight to a consideration of the nature and effect of their emergence in society at large.

Under the caption of "collective opinions and beliefs" are passed in review his stock doctrines regarding the influence of racial character in the formation of opinions and beliefs; the importance of social environment, tradition, and custom in building opinions and beliefs; the peculiarities of opinions and beliefs as held by crowds; and the relation of the mind of the individual to the mind of the group, especially in crowds.[83]

In the division of his work devoted to a consideration of the "propagation of beliefs and desires" he deals with the spreading of opinions and beliefs by affirmation, repetition, example, and prestige; the influence of mental contagion in spreading opinions and beliefs; the significance of fashion in this process; the importance of literature and the press in the propagation of opinions and beliefs; and the effect of currents of opinions and their explosion, as in the emotional outbursts at the time of Peter the Hermit, Joan of Arc, Mohammed, Luther, and Napoleon.[84]

Finally, in discussing the subject of the "life of beliefs," Le Bon considers the intolerance and dogmatism of beliefs; the impotence of reason in the formation of beliefs; the maintenance of permanent beliefs by means of mental contagion and repeated suggestion; and the slow modification of beliefs, except by the contact of different cultures. He ends by maintaining that beliefs never die, but simply change their name according to the same principle as that of the transformation of energy and matter in physics.[85]

7. The Psychology of Revolutions

Le Bon's reflections on the psychology of revolutions, which are more or less present in all his works, are brought together and expanded in the volume entitled *La Révolution française*

[82] Drawn, as he says, from his previous studies, and said by Sighele and Tarde to have been mainly appropriated from their works.
[83] Ibid., pp. 168-193.
[84] Ibid., pp. 194-232.
[85] Ibid., pp. 233-268.

et la psychologie des révolutions.[86] He introduces this work with a brief discussion of what he calls "the psychological revision of history." His thesis is that the seeming difficulties of historical interpretation vanish as soon as one recognizes that the irrational and often involuntary beliefs, and not the rational and intellectual factors, have been the dominating influences throughout history. "The solution of the historical difficulties which had so long been sought was thenceforth obvious. I arrived at the conclusion that besides the rational logic which conditions thought, and was formerly regarded as our sole guide, there exist very different forms of logic: affective logic, collective logic, and mystic logic, which usually overrule the reason and engender the generative impulses of our conduct."[87]

In discussing the psychology of revolutions Le Bon first proceeds to classify and characterize them. A revolution is any sudden or apparently sudden transformation of beliefs, ideas, and doctrines. The real and enduring revolutions are those that transform the character of a people, but such transformations are normally so slow a process that the word evolution is more descriptive of them than the term revolution. The most important of all revolutions are the scientific. They alone are accomplished by rational factors and they are the only type which really advances civilization. Nevertheless, their gradual and undramatic character has caused their significance to be overlooked by the conventional type of historian. Political and religious revolutions, which, from their dramatic nature, attract the attention of historians, are not derived from rational influences, but from affective and mystic forces. This gives them their dynamic power, but also renders them likely to be violent, absurd, and futile. Religious revolutions are even more violent than political revolutions. In this type the participants cannot be disillusioned by the results, because the truth or falsity of their principles can only be demonstrated in another world. Religious revolutions also have the most important results of the two, for, while they do little or nothing to advance the intellectual factors of a civilization, they are the most influential medium in transforming the sentiments of a people. Religion, particularly when intensified during a revolution, gives a people a moral unity and cohesion which could be obtained in no other manner.[88]

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[86] Paris, 1912. English translation by Bernard Miall, N. Y., 1913.
[87] Op. cit., p. 15.
[88] Ibid., pp. 23-48.

Le Bon next analyzes the part that governments and the people play in revolutions. Governments are usually very feeble in opposing a revolution, giving way immediately and with little resistance. A wise and efficient government, however, may check a revolution by following a proper policy. The menacing Russian revolution after the Russo-Japanese War was thwarted by the action of the government in temporarily conciliating the discontented masses and then exterminating or exiling the fanatical leaders. Governments may at times attempt to produce a revolution by their own action, but they are rarely successful unless the national character is not yet sufficently developed to render an effective resistance to the change of institutions, as was the case with the revolutionary reforms in Russia under Peter the Great. Yet, however profound may be the apparent change in the system of government, such a revolution rarely has a serious effect upon the mental characteristics of a nation. "To create a revolution is easy, but to change the soul of a people is difficult indeed."[89]

The mental characteristics of a people play a prominent part in determining the nature of its revolutions. The more stable the mind and institutions of a nation in normal times the more violent are its revolutions, for such people are not adapted to making gradual non-revolutionary changes. A nation with a flexible, adaptable mind may not escape revolutions, but those which it experiences are usually slight and but the final stage in a long period of gradual changes. France is a good example of the former type of nation, England of the latter. Contrary to the belief of many historians, the people, strictly considered, never conceive or direct a revolution. They simply obey the dictates of leaders, though they give the aspect of violence to the movement. To be strictly accurate, the people must be differentiated when one attempts to describe the part played by masses in revolutions. The great body of peasants, tradesmen, and honest artisans, who form the solid and substantial element in the masses, take little part in the violence of revolutions. Those who are guilty of violence, under the guidance of obsessed leaders, are the "degenerates of alcoholism, and poverty, thieves, beggars, destitute 'casuals,' indifferent workers without employment—these constitute the dangerous bulk of the armies of insurrection."[90]

Le Bon finds that there are several special varieties of mentality prevalent during revolutions. Each individual may have

[89] Ibid., pp. 49-59.
[90] Ibid., pp. 60-74.

different traits of character made more prominent at various times owing to changes in the stimulation from the social environment. In revolutions the sentiments of hatred, fear, ambition, envy, variety and enthusiasm, which are normally more or less suppressed, are given full vent. One of the most prominent types of mentality developed by revolution is what Le Bon calls the "mystic mentality." This is characterized by the attribution of a mysterious power to superior beings or forces, which are incarnated in the form of "idols, fetiches, words, or formulae." It is at the bottom of all religious and most political beliefs and is especially important during revolutionary periods. Another influential type of mentality which is conspicuous in revolutions is the "Jacobin mentality." This is based upon the mystic mentality to which are added feeble reasoning powers and strong passions. The typical "revolutionary mentality" adds to the mystic and Jacobin traits chronic restlessness and discontent—the spirit of perpetual rebellion. Finally, there is the "criminal mentality" which characterizes the degenerate anti-social class which is normally restrained by the hand of the law. This type constitutes the majority of the savage and violent element in revolutionary mobs. When one considers that revolutions are conducted under the combined direction and impulse of mystic, Jacobin, revolutionary, and criminal mentalities he can be little surprised at their violent nature, their absurd direction, or their ephemeral results.[91]

Le Bon concludes his introductory and general treatment by a repetition of his overworked views regarding the psychology of peoples, crowds, assemblies, and leadership.[92] In the second part of his treatise he applies these already venerable conceptions to an interpretation of the French Revolution. While this analysis is at times most brilliant and suggestive, his method of procedure is open to the most severe criticism. He apparently reached certain general conclusions regarding revolutions from a study of the French Revolution, and then applied these views to an interpretation of this very period. Of course, this gives a high degree of apparent plausibility and concrete substantiation to his theories, but quite fails to impress the critical reader. He concludes that the general result of the French Revolution, in France, at least, was to substitute the tyranny of the state for the oppression of the individual monarch.[93]

[91] Ibid., pp. 75-101.
[92] Ibid., pp. 102-120.
[93] Ibid., p. 286. For his general summary of the psychology of the French Revolution, see pp. 326-330.

In the concluding portion of his treatise Le Bon discusses the after-effects of the French Revolution, particularly in regard to the development of democratic ideas. The Revolution produced a crop of idealizers and theorizers who have planned the complete democratization of society. The ideal of "equality," rather than the shibboleth of "liberty" and "fraternity," has received the greatest emphasis, and is at present the pivotal doctrine of socialism. Le Bon finds that there are two distinct varieties of democracy. One is that of an intellectual aristocracy under democratic forms; the other is the popular notion of democracy based upon the ideal of equality or, perhaps better, upon the hatred of superiority. This latter variety is in direct opposition to nature's principle of inequality, and has rarely received the support of great minds. Though the popular ideal of democracy is an illusion, it has great vogue because it is a belief. The Jacobin mentality has become general in Latin countries, as evidenced by the growth of anarchy, syndicalism, the hatred of superiority and restraint, and the incessant craving for the extension of state-activity. The distinctly new element in the modern situation is the struggle between capital and labor, which is an outgrowth of the Industrial Revolution. The vicissitudes, uncertainties, conflicts, and extension of duties in modern political life have compelled most states to develop an elaborate administrative system which constitutes the real government and threatens society with the tyranny of a caste of functionaries.[94]

8. The Psychology of the World War

It could scarcely be expected that so important and interesting an event as the World War would fail to elicit from Le Bon a psychological explanation, and before the conflict was half over he brought forth his *Enseignements psychologiques de la guerre européene,*[95] in which he set forth with great assurance a psychological interpretation of the causes and progress of the world conflict, based on his stock theories of social psychology. The only notable new element in this work is that the extreme laudation of Teutonic racial characteristics, which was so prominent in Le Bon's earlier works, has been replaced by a thorough-going acceptance of the view of the unique perversity of the Teuton, while his anti-patriotic bias against the French has disappeared in favor of a worship of

[94] Ibid., pp. 289-325.

[95] Paris, 1916, translated by E. Andrews as *The Psychology of the Great War,* N. Y., 1916.

French traits, as revealed by the war, which almost equals the adulation of Maurice Barrès. His main theoretical contention is that the war was primarily produced by psychological forces and that its genesis and course can be understood only by means of a psychological analysis. He stresses the importance of the affective, collective and mystic forces "forged in the dim realm of the unconscious," and holds that they have almost totally submerged the intellectual and rational factors and elements during the progress of the war. The conflict between psychic forces and tendencies in the world war is so deep-seated and comprehensive that it may produce a new psychological era in human development.

Le Bon views the war as a fundamental struggle between psychic forces, particularly those operating on an emotional and a sub-conscious level:

> The present war is a contest between psychological forces. Irreconcilable ideals are grappling with one another. Individual liberty is drawn up against collective servitude, personal liberty against the tyranny of State Socialism, old habits of international integrity and respect for treaties against the supremacy of the cannon. . . The present contest has more than one analogy with the religious wars of olden times. It is begotten of the same illusions and shows traces of the same incoherent frenzy and brutality. It is ruled exclusively by irrationality, for if reason had been able to dominate the aspirations of kings and nations, there would have been no war today. . . Never in the course of the ages has there, perhaps, been a better opportunity of seeing how men's conduct is sometimes dominated by unconscious influences whose pressure is so great that no will can withstand them.[96]

It is interesting and amusing to note that in this work the German people are represented as the exponents of state socialism and collectivism, while the French are pointed out as the champions of individual liberty and personal initiative—a complete reversal of the rôles assigned to these two nations in Le Bon's earlier works.

Le Bon's present anti-German bias appears most clearly in his analysis of the causes of the war, in which he places the burden primarily upon the Germans, whom he believes to be intoxicated with the delusion as to their superiority and their mission to rule the world:

> The victory of the Teutonic theory of the absolutism of force would carry the nations back to the most distressful periods of their history, back to the eras of violence when the law of the strongest was the sole foundation of justice. . . Like the Arabs of Mohammed's day, the Teutonic nations are deluded by a dream which

[96] Op. cit., pp. 18-20, 173.

makes them fancy that they are a superior race, destined first to conquer the world and then to regenerate it.[97]

Le Bon is now disposed to contest vigorously this view of German superiority:

The German of our day is not by any means the transcendent creature whom the vanity of his historians has imagined him to be. He is the heir of the men whom Napoleon conquered at Jena with such ease, and he has nothing really superior about him except an exceedingly strict discipline and a meticulous organization which is well adapted to the needs of the present era.[98]

Yet so deep-seated is this mystic German illusion as to the imminence of Teutonic world leadership that it will take more than one defeat to free the German mind of this national psychosis:

Whatever success Europe may win in the end over Germany's attempt at hegemony, there is no hope that it will be lasting, for the ideal of domination is one of those mystic beliefs whose duration is never brief. A nation which has been chosen by God to conquer and regenerate the world does not readily abandon such a mission, and Germany will not relinquish it until she has been defeated many times.[99]

Holding that the influence of rational and intellectual factors over group action is almost wholly an illusion, Le Bon turns to analyze what he believes to be the really fundamental causative psychological elements in producing the Great War. He classifies these as affective, collective, race-psychological and mystic. The affective forces are the chief creator of race-hatreds:

Affective forces are among the great regulative forces of history. By the strength with which they clothe our ideas they lead us to look at things in different ways, according to the varying degrees of our sensibility. All nations possess an aggregate of inherited feelings, which are determinative of their mental orientation, which cause individuals of unlike ancestral equilibria to take different views of the same questions, and which occasion those inextinguishable race-hatreds that are among the chief causes of the European War.[100]

Added to these racial antipathies are those forces which arise from the collective psychological influences which produce that crowd-psychological state to which Le Bon has devoted

[97] Ibid., p. 19. Le Bon apparently does not recall that a study of his own writings by Germans would have contributed materially to this inflation of the Teutonic national ego which he has described.
[98] Ibid., p. 465.
[99] Ibid., p. 468. For Le Bon's equally changed views on French national character, see pp. 21, 466ff.
[100] Ibid., p. 30.

so much attention. This comes to the front in times of excitement like war and destroys all that rational self-control which may exist in normal periods:

> In ordinary times the intellectual forces of a country's best elements easily predominate over its collective forces, but in great crises, such as wars, revolutions, and the like, this is not the case; for the collective forces, which are derived from individual influences, are then capable of becoming so powerful as to sweep whole countries into an irresistible whirlpool, and to cause the emergence of new manifestations of the national mentality.[101]

This war mentality is not only produced by contemporary agencies, but is also strongly reinforced by tradition and those cultural factors which Le Bon groups together as race-psychology or national mind. Any nation has its view of its neighbors shaped by the hatreds of past generations as well as by present differences. "It is no exaggeration to say that the fiercest fighting upon our fields of battle is due to the innumerable hosts of the dead more than to the living."[102] But of all the non-rational factors determining group conduct today the most powerful are the mystic forces. Especially threatening is the mystic Teutonic obsession of world domination:

> Generally speaking one may say that mysticism is characterized by a taste for mystery, love of the supernatural, contempt for experience, and a belief that superior powers intervene in mundane phenomena. . . . The mystic forces, which science long disdained or knew not, rank foremost among the motives which rule mankind. . . Motives of mystic origin have always been the strongest of mankind's various incentives; for it is they which have created the illusions that quicken history, they under whose influence great empires have been destroyed and others founded, and they upon which even now rest the foundations of civilization. The modern world deems itself free from their sway, and yet humanity has never been more enslaved by them. . . If Europe is today in conflagration, if the flower of our youth is dying on bloody battle-fields, and if countless families are left desolate, it is because one nation believes itself destined to regenerate the world, upon which it means to impose its own mystic chimera of universal domination.[103]

After this preliminary theoretical analysis of the chief psychological factors involved in the war, Le Bon applies them to an explanation of the diplomatic negotiations which immediately preceded the war;[104] to the clarification of the methods and processes of modern warfare;[105] to the exposure of the

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[101] Ibid., pp. 34-35.
[102] Ibid., p. 35.
[103] Ibid., pp. 37-40.
[104] Ibid., pp. 177-279.
[105] Ibid., pp. 283-361.

psychic basis of German terrorism;[106] and to a forecasting of the new problems which may grow out of the World War.[107] He holds that the terroristic methods of the Germans well demonstrate the slight influence which education has over character and collective emotion. Under the excitement of the collective-psychological state the ancestral barbarism breaks through the slight veneer of civilization. The unique nature of the German *Schrecklichkeit* Le Bon holds to be due to the fact that there was in Germany less restraint upon the primordial barbarism and that this barbarism was of a lower and more animal sort than in the rest of Europe. Hence it was but natural that the inevitable savagery of war would reveal itself most rapidly and most terribly in Germany.[108]

Le Bon believes that the World War is likely to prove one of those great cultural crises which alone seem able to bring into existence a new psychological era:

> It would seem that when nations reach a certain point in their history they cannot progress except under the influence of these great crises which are, perhaps, necessary for their release from the embrace of a past which clasps them too closely, and from habits and prejudices which have become too firmly established.[109]
>
> The European War marks the beginning of an era of upheaval in our manner of life, our feelings, and our thought. We have perhaps reached one of those historical periods in which, as at the time of the French Revolution, the ideals and principles of mankind are changed and a new aristocracy makes its appearance. The nations are being hurried towards a future which is not yet illumined by the faintest glimmer of light. Something they cannot foresee holds dominion over them, and political and moral ideas which they had considered incapable of change now seem destined to disappear. Theories and doctrines are vanishing one after the other, and no longer is the future assured, for the psychological forces which are locked in mortal combat are but beginning to work.[110]

9. The Psychological Changes Produced by the World War

In order to keep his social psychology thoroughly abreast of current historical developments Le Bon published a year before the Armistice a work on the psychological consequences of the World War, entitled *Premières conséquences de la*

[106] Ibid., pp. 365-407.
[107] Ibid., pp. 411-462.
[108] Ibid., pp. 381-398.
[109] Ibid., p. 463.
[110] Ibid., p. 23. Le Bon feels reasonably assured, however, that France will lose her dangerous illusions of the past and that the rest of the world will come to view Germany in her true light.

guerre: transformation mentale des peuples.[111] In the introduction to this work Le Bon reiterates the chief propositions set forth in his *Psychology of the Great War,* namely, that the war is one of the great cultural crises of history; that it was caused and is governed almost wholly by non-rational forces; that the most ominous of these forces is the mystic Teutonic conception of world-hegemony; that the war is the price which the world is paying for its past illusions; and that these illusions are now passing and a new cultural era is about to be entered.[112]

Le Bon contends that before the war the western world was as much dominated by illusions as ever before in its history. The only difference from earlier illusions was the fact that social and political illusions had replaced the religious. Among the more dangerous and fatal of the pre-war illusions were those of state socialism, and the pacifist illusion that individuals and states are controlled by rational factors and that wars could never take place again in so advanced a civilization as that of western Europe. The war has shown the reality of the complete sway of the emotional and the unconscious psychic factors and the potency of the mystic elements. These illusions not only existed before the war, but also continued for some time afterward. It was the German ignorance of crowd psychology which led them to commit such colossal blunders as the sinking of the Lusitania, the execution of Edith Cavell, and the Zeppelin raids on British cities. The war has also borne out the theory of the Romans, the Crusaders and Napoleon, that no great military success can exist unless the soldiers are caught up in some great mystic enthusiasm which gives them a superhuman power and endurance. Experience gained during the war has wiped away most of these older illusions and has left the world wiser if much sadder for the grim lesson.[113]

From these general observations on the psychological changes occasioned by the war Le Bon turns to an analysis of the more obvious mental and social transformations which have taken place among these European states which have been at war. He contends that French degeneracy before the war was so great and deplorable that it can be fully comprehended only by a comparison with the heroism revealed in France by the crisis of the war. Before the war France was divided into selfish and competing economic and social classes; a general strike against any war was threatened; state activity and

[111] Paris, 1917. No English translation has yet appeared.
[112] Op. cit., pp. 1-12.
[113] Ibid., pp. 13-45.

state socialism had become a fetish; politics were paralyzed and corrupted by the inferior men drawn into the French parliamentary system. The war wrought a great transformation in French society. It brought to the front much abler leaders in politics; it improved the personality of men of indifferent capacity and gave a better moral fibre to the general body of the citizens; the common experiences of all classes and both sexes during the war produced an unprecedented social solidarity in France and secured a marked advancement in the status of women; a renaissance of religious interest was evident and a unique prevalence of religious toleration existed; and there was a search for a better and higher philosophy of life, but none could be found. Not only was there a great transformation within France itself, but also the position of France in the esteem of the world was greatly improved. The pro-Germanism which existed everywhere before the war melted away and France began to appear as the guardian of civilization.[114]

Germany appears to Le Bon to be the one nation that was not changed by the war in a psychological sense. The Germans seem to have retained the same illusion as to the superiority of the Teutonic race and its destiny to rule the world. Yet the Germans exhibited plenty of signs of degeneracy during the war if they had been willing to recognize them.[115] In Austria-Hungary the most interesting feature of the war was the grotesque propaganda and the intense repression of speech and news which were necessary to hold so diverse a people together as a unified fighting force.[116] Great Britain underwent a great transformation. Before the war she was isolated from the rest of Europe, was supreme on the seas, and had no fear of an attack. Hence Great Britain was more dominated by pacifism and less prepared for war than any of the major continental states. The facts of the war brought about a "terrible awakening" for the British people. They were slow in getting adjusted to the war situation and the military emergency because of their lack of preparation and because of the importance of precedent and tradition in English political life and theory. Great Britain is ruled more by her dead than other European states, and to get a new and adequate military organization she had to conquer not only the opposition of the living, but also the traditions established by the dead.[117] Russia in 1914 was on the eve of a complete

[114] Ibid., pp. 48-140.
[115] Ibid., pp. 141ff.
[116] Ibid., pp. 196ff.
[117] Ibid., pp. 211ff.

German conquest of its industry, commerce, education, administration and army. In fifty more years a Russian war with Germany would have been impossible, and even in 1914 the German penetration well-nigh paralyzed Russian military activities.[118] The war also vitally affected the neutrals. The United States was kept out of the war by the prosperity produced through the sale of munitions, by the German propaganda, and by the inability to develop a coherent national policy on the subject of intervention. Japan not only derived great economic benefits from the war, but was also able to carry forward Japanese domination in China to an unprecedented degree. This Le Bon regards as a fact of nearly as great importance to the world as the war itself.[119]

Le Bon closes his last work with certain reflections upon the future of international relations. He contrasts the operation of the principles of law and force in human society and in history. He finds that the Latin peoples are the great champions of the reign of law, while the Teutonic peoples support the rule of force—the principle dominating the animal world. Force and anarchy have dominated international relations in the past because no common power has been found with sufficient strength to enforce international law. International law in the past has been weak and inadequately enforced, and its operation has been further handicapped by the fact that the Germans hold that in war all laws are abandoned.[120] Nor do economic losses seem adequate to prevent wars. It had been shown at great length before 1914 that wars were not good economic investments, even for conquerors, but rulers and people alike think not of the probable losses in present or future wars, but of the alleged gains in wars of the distant past, and in any war, however expensive to the nation at large, many individuals and classes become wealthy therefrom.[121] The chief hope for the future pacific adjustment of international relations must rest upon that growing interdependence of nations which renders wars more repugnant and more costly. Yet one can hope for ultimate peace and disarmament only when false ideas have been dispelled, and for this task many repeated experiences of war will be necessary, especially to teach the Germans the folly and hopelessness of their dream of world domination.[122] Les

[118] Ibid., pp. 222ff.
[119] Ibid., pp. 254ff.
[120] Ibid., pp. 268ff.
[121] Ibid., pp. 296-301.
[122] Ibid., pp. 302ff. Le Bon's immediate contact with the realities of war seems to have led him to modify radically his pre-war views as to the great social, moral and spiritual benefits of war.

expériences répétées finiront par enseigner aux peuples l'inutilite des guerres, mais nous n'en sommes pas encore là.[123]

10. Summary of Le Bon's Basic Theories on Social Psychology

Le Bon's salient doctrines may be summarized as follows: Every race possesses certain definite psychic traits built up by the slow accumulations of experience, and perpetuated by tradition. These psychic traits, rather than institutions, are the determining factors in civilization, the latter being simply an objective expression of the former. Among these psychic traits which constitute national character, or the soul of the race, the affective, mystic, and unconscious factors are the most powerful, quite overshadowing the conscious, rational and intellectual elements, though it is to the influence of the latter that progress is due. Without a coherent and unified group of psychic traits constituting the soul of the race, the civilization of that race cannot develop or be perpetuated. It is futile to attempt to change these fundamental psychic traits by a revolutionary or any other artificial transformation of institutions. Therefore, an excessive degree of state activity is worse than useless; law-making power should be confined to the codification of well-established and persistent customs; and government, in general, should be limited to that minimum of activity which is necessary to preserve order and secure the proper degree of mental discipline for the individual citizen. Both socialism and syndicalism are, thus, dangerous movements; the former wishing to procure excessive state activity, and the latter desiring to abolish the state altogether. Owing to such results of the Industrial Revolution as the growth of cities and the consequent concentration of population, the improvements in communication, and the extension of the suffrage, modern political life has tended to become dominated by crowds. The crowd is abnormal in its psychological characteristics, being highly emotional, exceptionally weak intellectually, and exceedingly susceptible to suggestion. It is easily guided, however, by leaders possessing prestige, who, to be successful, make use of those principles of affirmation, repetition, contagion, and imitation, whereby a crowd may be persuaded and convinced. Hence, it is highly essential that society shall assure the highest quality of leadership for crowds, and thus be able to direct their dynamic energy into activities which are conducive to the public welfare. If this is not done, and crowds are left to the exploita-

123 Ibid., p. 312.

tion of shortsighted and selfish demagogues, they must continue to be a constant menace to the integrity, well-being and even the existence of modern society. The World War was produced by the domination of the mob or crowd mind, operating over national areas and submerging all rational factors and processes. In its most fundamental aspects the conflict was a psychological struggle between contending sets of national ideas and emotions. It constituted a great psychic upheaval and transformation, destined to bring in its wake a new psychological and cultural era. War can be eliminated only when society is brought under the control of that leadership of the real intellectual aristocracy which is needed to guide the crowd mind in times of peace. Only under such leadership can society be brought to understand the growing and vital interdependence of nations, international relations be brought under the control of legal forms and processes, and those false ideas regarding the biological and social benefits of war and the conquering mission of any nation be forever destroyed.[124]

[124] Le Bon has summarized his theories in a little volume entitled, *Aphorismes du temps présent.*

A PSYCHO-ANALYTICAL STUDY OF EDGAR ALLAN POE

By Lorine Pruettf

The life of Edgar Allan Poe might be considered an unhappy record of that "disaster" which "followed fast and followed faster" this man of brilliant capacities till it drove him into opposition with most of the world, deprived him of the love he so inordinately craved, paralyzed his creative abilities, seduced him to seek a vague nepenthe in the use of drugs and stimulants, and, its relentless purpose achieved, cast him aside, a helpless wreck, to die from the darkened tragedy of a Baltimore saloon. Without further following such an anthropomorphic conception of fate, we must be impressed that both environmental circumstances and natural inheritance seem to conspire to cast the young poet in a rôle that is both sombre and wild, with a beauty that chills even more than it saddens.

The psychoanalyst who seeks to probe into the earliest details of life to find there the causes of many of the associations and complexes, which even then shadow forth the developments of later years, will be troubled in the case of Poe with both scanty and conflicting data. The attempt has been made in this paper to follow those accounts which seem to be stamped with the strongest degree of authenticity or at any rate probability.

Edgar Poe was born in Boston, January 19, 1809, while his parents were playing in a local theater. They were at that time very poor, so that his birth, in spite of distinguished ancestors, may be considered lowly, both from the standpoint of wealth and of the social status which players then occupied. Yet from his paternal grandfather, a general of revolutionary fame, he boasted of high lineage, and he consistently asserted his pride in being descended from a woman at once so beautiful and so noble as his mother, who both honored and was honored by her profession. Such assertions as these suggest that he spoke quite as much to assure himself as to convince others. While the reality allowed him full right to take pride in his descent, yet circumstances so contrived to cloud over

that reality as to make his proud and sensitive spirit constantly alert against the possibility of an indignity.

The father of Poe was a victim of consumption, as was later his cousin-wife. Soon after the father's death, the mother died in abject want in Richmond, leaving her three small children to the mercy of strangers. Edgar was then two years old. The orphans were adopted by different families and seem to have known very little of each other. The elder brother William was, to quote from the words of a cousin, J. P. Poe, "a man of taste and genius, and wrote many fugitive verses, which have been lost, but which are said to have exhibited poetical power of a high order." He was not averse to the flowing bowl, and after his rejection as a lover, went to sea, where through recklessness he got into a sailor's scrape; he died at the age of twenty-six, leaving behind him the reputation of great but wasted talents. The youngest child, Rosalie, was so hopelessly dull that she could never attain proficiency in anything at school; she was utterly incapable of procuring her own maintenance, and after the family which had at first taken her in ceased to befriend her, she led for many years a precarious existence till she was finally admitted to a charitable institution in Washington. There she was credited with many eccentricities; she died at the age of sixty-four. Samuel Poe, a notable oddity of Baltimore, is said to have been the poet's uncle. His father, educated for the law, found such an existence unbearable, and after several rebellions separated himself from his family, marrying the English actress, Elizabeth Arnold, and adopting her profession. These facts seem to indicate a decided neurotic taint in Poe's paternal inheritance. This inferior nervous system predisposed many of the family toward flights from reality, alcoholism being their favorite form of erethism. Practically nothing is known of Poe's maternal inheritance, Elizabeth Arnold having been an orphan born at sea. She is said to have been extremely talented in singing, acting and painting. Edgar inherited her artictic ability, was very clever at drawing and passionately fond of music. Along with these gifts of the muses he inherited also those characters which were to bring him at last to an end sadder and more terrible than even that of his young actress mother, starving in Richmond.

A few weeks after Mrs. Poe's death, the Broad Street theater where she had been acting, was consumed in the awful conflagration of Christmas Eve, 1811. The death of so many distinguished people caught in this fire-trap sent a thrill of horror through the United States and was discussed in hushed

tories by the Virginians for many years thereafter. The story of this event and the sight of the burnt building where his beautiful mother had so often graced the stage, must have had a powerful effect upon the dawning imagination of young Edgar, so that even at this early age there appeared to him the trilogy which so possessed his mind in after years—death, love, and beauty.

Now a tubercular father and cousin, an eccentric uncle, a drunkard brother as well as many relatives known for a "too free use of the bottle," and an imbecile sister, coupled with the peculiar genius of the poet himself, his sensitiveness to the effects of stimulants and temporary fits of insanity toward the last of his life, form fairly conclusive evidence that there was in the Poe family a decided organic inferiority. The lesion on the brain from which Edgar suffered in later life may have been either the result of syphilitic infection or apoplexy, or caused by an inherited inferior brain for which, according to Lombroso, genius is an over-compensation. (1, b.)

On the death of his mother, Edgar was adopted and baptized into the family of John Allan, a Virginia planter. From the childless wife of Mr. Allan the boy received considerable affection, though it is improbable that she was ever able to give any real understanding to her brilliant foster son. The adoptive father seems to have regarded him with an ambivalent feeling of good and ill will. In early years he was undoubtedly proud of the boy's beauty and precocity, and delighted to have him in to entertain guests after dinner by reciting long passages of poetry. The strain of such occasions must have been severe to the sensitive, excitable child. It is in such festive gatherings that he is said to have acquired, at an early age, the taste for alcohol which was later to play such a sinister part in his undoing. Dr. Bransby, under whom he studied in England for five years, described him as "a quick and clever boy" who "would have been a very good boy if he had not been spoilt by his parents," who "allowed him an extravagant amount of pocket-money, which enabled him to get into all manner of mischief." (7) His early boyhood, then, was passed as the spoiled child of indulgent parents, who gave him pocket money rather than love and sympathy. Poe himself, in speaking of his foster parents, says that he never received the parental affection or family sympathy for which he longed. But according to his biographer, John H. Ingram, "Throughout life a morbid sensitiveness to affection was one of Poe's most distinguishing traits," and it is highly probable

that any normal affection would have seemed insufficient to the neurotic boy.

The Allans sailed for England in June, 1815, taking with them their adopted son. Edgar was placed at school under Dr. Bransby in Stoke-Newington, then a suburb of London. This historic old place, with its shadowed walks and memories of great and ill-fated lords and ladies, became the home of the child of genius during the next five years; years in which the childish imagination was quickening into life, fed on the lore of the classics and the pervading atmosphere of antiquity; years in which the passionate love of beauty turned to the loveliness of the old English town for gratification; years, too, in which the first attempts at verse-making were begun. It goes without saying that had the sensitive boy spent those five impressionable years in a different atmosphere—had he then known a normal home life and formed the normal associations toward a father and mother—his story must have been vastly different. His absence from the Allans at this period definitely precluded the establishment of ties of affection which might in later years have changed both his attitude and theirs. In 1820, again in the summer, Edgar returned with the Allans to America. The importance to the dreamy child of these two long sea voyages must have been tremendous. Sea voyages predispose to introspection, and six weeks at sea on these two occasions provided a great stimulus to the boy's imagination and love of the beautiful. Many of his stories show clearly the effect of the sea upon him, and the close observation which he must at some time have given it.

Returning to Richmond Edgar Allan entered a fashionable preparatory school. As he grew older he came to realize the anomalous position which he occupied in the Allan household and among the arrogant, aristocratic sons of Richmond. "His supremacy in intellectual training and his easy physical prowess made him the most illustrious school boy in Richmond, but he was not allowed to derive pleasure from this high eminence. His playmates, too well trained in genealogy and taught an extravagant pride of ancestry, did not let him forget that his mother was an actress and that the privileges he enjoyed and they envied were owed to the beneficence of a Scotch merchant. These reminders of his inheritance and environment forced him into an unnatural moodiness and deprived him in large part of that frank and friendly companionship based upon a sense of total equality." (9b, Intro.) Such a situation, of course, increased Edgar's rebellious pride, made him feel the necessity for defending himself against

the slightest suggestion of inferiority. Feeling or imagining himself either tolerated or scorned steadily intensified his desire for superiority. He became a daring swimmer, took great risks to show himself above others, and would endure no implication that any one was his equal in his chosen sport. His early fondness for Byron possibly led him to make this identification with the British poet who was also noted for his prowess as a swimmer. His leadership in intellectual pursuits must have caused him to incur still further the enmity of his schoolmates. He appears to have made no effort to ingratiate himself with the young aristocrats; he would be sought rather than seek others. One of his schoolmates at this time says, " Poe, as I recall my impressions now, was self-willed, capricious, inclined to be imperious, and though of generous impulses, not steadily kind, or even amiable; and so what he would exact was refused to him." (7) The result of such a strained situation was to be expected. The adolescent boy withdrew more and more within himself, seeking consolation in his own dreams and mental imagery, so that by the time he reached the University of Virginia, at the age of seventeen, he is spoken of as having many noble qualities and being endowed by nature with great genius and diversity of talent but with a retiring disposition and possessing few intimate associates.

Poe was registered at the University of Virginia from February 14 to December 15, 1826. He was in good standing with the faculty and obtained distinction at the final examination in Latin and French, then the highest honors to be obtained. It had been the theory of Jefferson, whose creation the new University was, that there should be no restrictions on the students, other than the expectation that they would conduct themselves as gentlemen. The young bloods threw themselves with zest into the freedom of such a life; most of them wealthy and of high lineage, they set themselves to lead the life of reckless extravagance, of mingled bravado and chivalry, which they considered characteristic of a gentleman at that period. Gaming and drinking were indispensable to their ideas of what a gentleman should do. Poe fell readily into both these diversions, gambled recklessly and left the University owing $2,000 as debts of honor. In his drinking he was noticed to drink a glass of punch at a gulp, apparently craving the stimulation rather than the flavor of the drink. One glass, too, was said to be all that he usually desired. This habit of drinking *en barbare,* as Baudelaire calls it, he kept up all through life. (12, p. 33.) At the University, he

was known for his ability in writing extravagant stories, as well as in verse-making.

The organic inferiority of both lungs and mind, if we follow the theories of Adler, demanded compensation, which the youth found in drawing and in writing stories and poems. (1, b.) His " will to power," as we have seen above, would brook no superior, nor even equal, in either physical or mental pursuits, and it was this intolerance of the claims of mediocrity which brought upon him in later life the enmity of much of the literary world. He, himself, was known to both deny and affirm his great ambition, but his ideal goal of superiority as a " litterateur " must be considered the " guiding fiction " of his life. In his poems and in his stories he consistently narrowed his attempts to the one field in which he was preeminent, the depicting of beauty and horror. His insistence, as a critic, that beauty is the sole motive of poetry, may be regarded as an attempt to place the stamp of critical approval on that which he himself did best. His feeling of " degradation " and of " inferiority " fired him with the passionate determination to be " on top." If his " masculine protest " could be satisfied in no other way he turned to the degrading of others, as witness his attacks on Longfellow and other poets as plagiarists. Poe himself believed that his absolutely unswerving devotion to truth was responsible for his scathing criticisms, but even when true, as they generally were, such criticisms represented not an abstract devotion to truth, for which he was willing to suffer deprivation and hardship, but were rather due to the pressure of his own guiding fiction, striving toward the maximation of his ego-consciousness. With women poets, Poe was seldom, almost never, critical. His desire for superiority seemed with women to take an entirely different form. He had the characteristic over-valuation of the opposite sex which, according to Adler, is invariably connected with the neurotic constitution. (1, a.)

The conclusions of Brill, based on studies of only or favorite children are quite applicable to Edgar Poe. Brill found that the adult only child shows one prominent feature, namely, he is a very poor competitor in the struggle for existence. (2.) After leaving the University the young Poe entered Mr. Allan's office, where he found the work intolerable. Just as his father had done before him, he shirked facing the hard facts of prosaic, everyday life, to seek the freer atmosphere of the artistic world, where he could obtain greater gratification for his egoistic impulses. Going to Boston, the city his mother had loved and where she said she had found her " best and

truest friends," he published a thin volume of youthful poems which attracted little attention. He was then nineteen and some of the poems had been written when he was twelve. Unable to support himself by literary endeavor in the city of his birth, he joined the army as a private, where he distinguished himself by his exemplary conduct and became a sergeant-major at the age of twenty. Lieutenant Howard testified later that "his habits are good, and entirely free from drinking;" Captain Griswold considered him "highly worthy of confidence;" Colonel Worth said that his deportment was "highly praiseworthy and deserving of confidence." (6, p. 71.) His foster father then secured him an appointment to West Point, which he resigned in five months. A. B. Magruder, a contemporary, writes: "He was an accomplished French scholar, and had a wonderful aptitude for mathematics. . . . He was a devourer of books, but his great fault was his neglect of and apparent contempt for military duties. His wayward and capricious temper made him at times utterly oblivious or indifferent to the ordinary routine." (6, pp. 84-5.) His resignation not being accepted, and finding the life of the cadet so irksome, he deliberately infringed the rules and brought upon himself a courtmartial and dishonorable dismissal. His conduct and his foster father's speedy remarriage after the death of Mrs. Allan in 1829 definitely severed his relations with Mr. Allan and removed any possibility of his securing the inheritance which he had been brought up to expect. From this time on his life is a weary record of struggles for the bare necessities of life. At the time when his first tale was accepted he was in such a deplorable state as to be too ashamed of his clothes to appear in decent society. In spite of his acknowledged genius and great industry, he was condemned to the life of a very poorly paid hack writer and magazine editor, was never able to make any headway towards securing anything approaching financial independence, his young wife dying without even enough covering for her bed.

Brill further found that the only boy, constantly associating with grown-ups, "is usually precocious even in childhood, and as he grows older he finds it very hard to associate with persons of his own age." (2, p. 258.) Bohannon by his extensive studies of only children confirms this judgment. He says, "134, out of a total of 269, get along badly with others, 54 only fairly well, while only 81 seem to be normal in their social relations. When they disagree with other children it is usually because of a desire to rule. If they fail in this desire they are likely to refuse to associate with the children

who cause the failure, and in a measure succeed in the wish to have their way, either by choosing younger companions whom they can control, or older ones who are willing to grant indulgence." (3, p. 489.) While Poe impressed many with his brilliancy and charm, his erratic conduct constantly estranged from him even those friends he did make—and he seemed to make enemies far more easily than friends. He had the characteristic tendency of the neurotic of beginning life anew with each new friend and each change of circumstance, feeling a new enthusiasm and another opportunity, which he lost through the vagaries of his own conduct. In this connection his will to power manifested itself through his frequent reiteration that he had thousands of friends. Bohannon found precocity also to be the most prominent trait of only children. Poe's precocity has already been mentioned. It seems probable that no other English poet has written at so early an age such good poetry.

Likewise Brill found that the only child, through the coddling of his parents, is in adult life unable to bear the slightest depreciation. Poe showed this characteristic time after time in his undignified replies to criticisms which a more evenly balanced nature would have ignored. Also the only child develops into a confirmed egotist and is conceited, jealous and envious. After precocity, Bohannon found as most prominent characteristics: selfishness, imaginativeness, affection, jealousy, mental defects, temper, self-will, vanity—all of which are more or less exemplified in the character of Edgar Allan Poe. His depreciation of so many of his literary brethren has already been noted. He himself declared, "my whole nature utterly *revolts* at the idea that there is any Being in the Universe superior to *myself!*" (7.) We see, then, the years of his life spent as the spoiled child of the Allans combining with his neurotic inheritance to effectively inhibit his making the necessary adjustments to the demands of reality. Bohannon's studies trace a further parallel. He found the only child has a less healthy and robust constitution, more commonly suffers from mental and physical defects, his social relations are characterized frequently by friction and his peculiarities more pronounced. Finally, Brill concluded from a study of 400 patients that the "majority of only children do not marry at all or they marry some near relative whom they unconsciously identify with their parent image." (2, pp. 260-1.) Edgar Poe also fits into the scheme, he marrying his first cousin, Virginia Clemm, although it seems doubtful if he married her for the above reason so much as because he was able to find

security in the maternal love of her mother, as perhaps an unconscious manifestation and inhibition of the incest desire.

In considering the love life of Poe there were so many women whom he addressed in enraptured terms of adoration that it is difficult to know which, if any, had any lasting influence upon his development. His mother was to him an idealization of feminine charms, about as tangible as the vanished fragrance of unseen flowers. His boyish poet-soul, musing over the memory of a beautiful and unknown mother, whose tragic fate could not but win his sympathy, and clothing her image in all the matchless virtues of a fertile imagination, created around her name the first of those sadly lovely and unreal women who move softly through his stories and poems.

His foster mother provided his wants and even luxuries as well as some affection, but seems in no way to have satisfied his passionate desire for love and approval. It is significant that, as one of his schoolmates remarked, he was never known to take any boy into his home, always preferring to go to theirs. While in school in Richmond, at the age of fourteen, he met Mrs. Helen Stannard, the mother of a boy friend, and on her speaking kindly to him, became at once her humble adorer, offering to her the white flame of his adolescent worship. She became the confidant of all his boyish sorrows and the redeeming influence of his turbulent and passionate youth. On her death he felt himself intensely bereaved and could not endure the thought of her lying lonely in her tomb in the neighboring cemetery. So for months he is reputed to have gone nightly to the graveyard for solitary vigils by her tomb, keeping warm the memory of her who had been kind to him. His analytic mind showed always a peculiar fascination for the secrets of the tomb, a desire to probe into the last hidden process of life's disintegration. His feeling regarding his friend's loneliness was but one example of the idea by which he was haunted through life: that the dead are not wholly dead to consciousness. This theme is repeated many times in his writings; for instance, in the revival of his dead wife in the tale "Ligeia," or in the terrible return of the Lady Madeleine in "The Fall of the House of Usher." His broodings in the darkened cemetery by the tomb of the one person he felt had understood him must have laid a foundation for much that was weird and abnormal in his after life. The melodious poem, "To Helen," was inspired by the memory of this lady, whom he called "the one idolatrous and purely ideal love" of his boyhood. At another time he said, "The boyish poet-love is indisputably that one of the human senti-

ments which most nearly realizes our dreams of the chastened voluptuousness of heaven."

As a youth in Richmond he fell in love with S. Elmira Royster who lived opposite the Allans. She remembers him as a beautiful boy whose general manner was sad, " warm and zealous in any cause he was interested in, being enthusiastic and impulsive." The two young people became engaged, but her father, thinking her too young, intercepted all the poet's letters from the University of Richmond, and not until a year or so later when she became Mrs. Shelton did he learn why his passionate appeals had met with no response. It was at this time that Poe left Richmond and the office of Mr. Allan to seek his fortune in Boston, and it is entirely possible that the hurt to his vanity, to his insistent "guiding fiction" that he must be supreme, drove him away that he might avoid seeing the girl he desired possessed by another. In his youth he appears to have loved two other young women, one a cousin, Miss Elizabeth Herring, the other Miss Mary Devereaux, both of Baltimore, where he went after leaving West Point. Mordell (9) calls attention to the fact that by the age of twenty-three he had lost his mother, his foster-mother and Mrs. Stannard by death, and had parted from three sweethearts. These deaths and rejections Mordell held to be the cause of Poe's preoccupation with the subject of the death of beautiful women. It would seem, however, that such cause must be sought in the peculiar quality of the poet's own temperament rather than in external circumstances. The happenings of his early life undoubtedly must have conditioned his emotional reactions, but the close connection between love and death seems to have been the particular obsession on which his neurotic temperament fastened itself.

At the age of twenty-seven Poe married his fourteen-year-old cousin, Virginia Clemm. For some time previously he had made his home with his aunt, Mrs. Clemm, and from an early age Virginia had adored him. The attachment between him and his aunt was always very strong, she laboring and suffering for him as would a mother for an only son. Virginia's beauty and grace and sweetness, as well as her talent for singing, identified her with the image of the young and beautiful mother he had never known, while the maternal care and devotion of Mrs. Clemm offered him a refuge and safety from the troubles and disapproval of the outside world. His mother-image, being a creation of his own imagination, may be said to have split to take in the two personalities of his wife and mother-in-law—the one a radiant young creature,

satisfying his esthetic cravings, the other tender and untiring in maternal devotion satisfying the neurotic craving for protection. In later life Poe found many women friends, women of the highest attainment and character, who were his warmest defenders and for whom he expressed the deepest sentiments of affection. Among these were Mrs. Osgood, Mrs. Richmond, Mrs. Lewis and Mrs. Helen Whitman. From the peculiar character of his "will to power" it is doubtful if individual women ever influenced him much. His nature demanded the adoration and approval of "woman," rather than sexual conquests, and he worshiped in his poems a feminine idealization to which he ascribed various names. These women are never human; they are not warm flesh and blood, loving, hating or coming late to appointments—they are simply beautiful lay figures around which to hang wreaths of poetical sentiments. His emotional interest lay in himself, rather than in outer objects; he wished to be loved, rather than to love.

The poetry of Poe reveals two things, a very considerable degree of introversion (in the sense in which Jung uses the term) and a flight from reality. His poems are to an unusual degree "Out of Space—out of Time." Where a Byron or a Shelley revolted against political injustice and became the ardent apostles of liberty, Poe passed serenely through the troublesome years of anti-slavery agitation apparently untouched by the passions of those around him, worshipping only the Beauty whose expression is Art, interested only in the inner conflict within his own soul. In his themes he is neither American, nor Virginian, nor of the nineteenth century. For him the world was depreciated till it scarcely existed; finding reality not to his satisfaction he fled to a world of his own creating. As he said in his story "Berenice": "The realities of the world affected me as visions, and as visions only, while the wild ideas of the land of dreams became, in turn,—not the material of my everyday existence—but in very deed that existence utterly and solely in itself." Barbey d'Aurevilly speaks of what he calls Poe's "sècheresse," the terrible dryness of his art, and says, "His intellect was real; everything else about him was exquisite feigning. His passion, his human sympathy, his love of nature, all the emotions that go into his fiction, have a counterfeit unreality." (5, pp. 127-8.) This view accords well with Jung's theory of the introvert who, interested in thinking rather than in feeling, assumes the conventionally correct emotions. Poe himself wrote: "In the strange anomaly of my existence, feelings with me *had never been* of the heart, and my passions *always were* of the mind." This last is in line with his general atti-

tude toward women which allowed him to love intellectually so many women, without craving the physical expression of that love. The Freudians would consider this the result of the damming of the libido, perhaps due to some early experience, and they would support their view by calling his stories of horror expressions of anxiety, which to them always has a sexual connotation. It is quite as plausible to accept his own explanation and to hold that his passions were largely of the mind, which, as noted above, is entirely consistent with the introverted type. Lowell, in his "Fable for Critics," confirms this, saying that Poe "has written some things quite the best of their kind. But the heart somehow seems all squeezed out by the mind." Poe wrote to Lowell at the time when he was thinking he had found in the New England poet a congenial spirit: "I have been too deeply conscious of the mutability and evanescence of temporal things to give any continuous effort to anything—to be consistent in anything. My life has been *whim*—impulse—passion—a longing for solitude—a scorn of all things present, in an earnest desire for the future." Mrs. Whitman, in her introductory letter to Didier's Life of Poe, says that "his proud reserve, his profound melancholy, his *unworldliness*—of nature made his character one very difficult of comprehension to the casual observer."

The small volume of verse on which rests Poe's substantial claims to poetic genius, represent the work of his life which he was constantly refining. He composed with the care and effort of the introvert, revising his poems many times, usually to their improvement, intent on the perfection of quality rather than quantity. His themes are few, not from any paucity of imagination but from a complete absorption in a few dominant ideas. He sought, not the varied pleasures of the world, but the interpretation of Beauty alone, the highest form of which he felt to be linked always with melancholy. In "The Assignation" he says "there still lurked (incomprehensible anomaly!) that fitful stain of melancholy which will ever be found inseparable from the perfection of the beautiful." He constantly moved in his poetic imaginings among

> " . . . the ideal,
> Dim, vanities of dreams by night—
> And dimmer nothings which were real—"(Tamerlane)

In struggling with the hard, unlovely realities of life, he cried out

"Oh! that my young life were a lasting dream!
My spirit not awakening, till the beam
Of an Eternity should bring the morrow.
Yes! tho' that long dream were of hopeless sorrow,
'T were better than the cold reality
Of waking life, to him whose heart must be,
And hath been still, upon the lovely earth,
A chaos of deep passion, from his birth."

And later in the same poem entitled "Dreams":

"Dreams! in their vivid coloring of life
As in that fleeting, shadowy, misty strife
Of semblance with reality which brings
To the delirious eye, more lovely things
Of Paradise and Love—and all our own!
Than young Hope in his sunniest hour hath known."

In "A Dream Within a Dream" we find rebellion against the disappointments of life.

"I stand amid the roar
Of a surf-tormented shore,
And I hold within my hand
Grains of the golden sand—
How few! yet how they creep
Through my fingers to the deep,
While I weep—while I weep!
O God! can I not grasp
Them with a tighter clasp?
O God! can I not save
One from the pitiless wave?
Is *all* that we see or seem
But a dream within a dream?"

In still another poem called "A Dream" is seen the contrast between what he has and what he has wanted, between the real and the ideal world of fancy:

"In visions of the dark night
I have dreamed of joy departed—
But a waking dream of life and light
Hath left me broken-hearted."

In his "Sonnet—To Science" he calls science the vulture which has

" . . . torn the Naiad from her flood,
The Elfin from the green grass, and from me
The summer dream beneath the tamarind tree."

The shimmering irridescence of "Al Aaraaf," that mosaic of sensuous beauty of sight and sound, the music and glowing images of which haunt our senses even while the mind does not comprehend it, opens with the description:

"O! nothing earthly save the ray
(Thrown back from flowers) of Beauty's eye
As in those gardens where the day
Springs from the gems of Circassy—
O! nothing earthly save the thrill
Of melody in woodland rill—
Or (music of the passion-hearted)
Joy's voice so peacefully departed
That like the murmur in the shell,
Its echo dwelleth and will dwell—
Oh, nothing of the dross of ours—
Yet all the beauty—all the flowers
That list our Love, and deck our bowers—
Adorn yon world afar, afar—
The wandering star."

Of his "Politian" he says:

"He is a dreamer and a man shut out
From common passions."

He speaks to the singing Israfel:

"Yes, Heaven is thine; but this
Is a world of sweets and sours;
Our flowers are merely—flowers,
And the shadow of thy perfect bliss
Is the sunshine of ours."

He is constantly manifesting in his poems the desire to flee the imperfections of this world. But even in his dream world he is sad; he loved melancholy and kept her ever close to his side. In his "Fairy-Land" are

"Dim vales—and shadowy floods—
And cloudy-looking woods,
Whose forms we can't discover
For the tears that drip all over."

His "City in the Sea" is a picture of beauty desolated, of death reigning in the courts of life and love:

"There open fanes and gaping graves
Yawn level with the luminous waves
But not the riches there that lie
In each idol's diamond eye—
Not the gaily-jewelled dead
Tempt the waters from their bed;
For no ripples curl, alas!
Along that wilderness of glass—
No swellings tell what winds may be
Upon some far-off happier sea—
No heavings hint that winds have been
On seas less hideously serene."

The poems of Poe are songs of sorrow: beauty is in them, most often dead beauty, love is there, most often the love of those who are dead to him, and madness is there, as if the expression of the prophetic powers of his unconscious. Often enough, in moments of extreme depression, under the influence of drugs or in the temporary insanity induced by the use of stimulants, must he himself have felt those "evil things, in robes of sorrow," which "Assailed the monarch's high estate." The "Imp of the Perverse" came to him in actual life, and forced him to make appearances which he could neither have desired nor have calculated to benefit himself. His behavior in Washington which lost him the government appointment he desired, and his gratuitous insult to the Bostonians when, on being asked to lecture, he delivered a poem he had written as a child, must be considered as the perverse manifestation of "the will to power" which is gratified by putting a depreciation on others through one's own unbecoming conduct. This same striving for superiority is evidenced in "Israfel:"

> "If I could dwell where Israfel
> Hath dwelt, and he where I
> He might not sing so wildly well
> A mortal melody;
> While a bolder note than his might rise
> From my lyre above the skies."

Poe's heroes are largely autobiographical; they are melancholy men, pursued by unrelenting fate; they are neurotic, hypochondriac, monomaniac, victims of vain delusions; they are the prey of melancholia, insane from sorrow or from the thirst for revenge. In "Eleanora" he seeks to reassure himself, to take the proudly characteristic attitude of defiance to the views of the world. He writes: "Men have called me mad; but the question is not yet settled, whether madness is or is not the loftiest intelligence: whether much that is glorious, whether all that is profound, does not spring from disease of thought,—from *moods* of mind exalted at the expense of the general intellect." In the study of his own diseased thought he is distinctly psychological. He writes: ". . . . what the world calls 'genius' is the state of mental disease arising from the undue prominence of some one of the faculties. The works of such genius are never sound in themselves, and, in especial, always betray the general mental insanity." (6, p. 230.) This appears to be the artist's foreshadowing of some of the views of Lombroso and Adler, as if Poe himself felt that only through his own defects was he able to secure that superiority his soul demanded.

Poe's favorite poem, "The Sleeper," is occupied with his dominant theme, the linking of sex and death. "All Beauty sleeps!"

> "The bodiless airs, a wizard rout,
> Flit through thy chamber in and out,
> And wave the curtain canopy
> So fitfully—so fearfully
> Above the closed and fringed lid
> 'Neath which thy slumb'ring soul lies hid."

This is only one of the many pictures he has given us of the couch of beauty which is also a bier. "To One in Paradise," the "Sonnet to Zante," "Lenore," "Ulalume," "Annabel Lee" and the world-famous "Raven" are concerned with reflections over a beautiful woman who is loved and dead. "Annabel Lee," written after his wife's death, is reminiscent of his experiences as a boy when he kept his lonely vigils in the cemetery beside the tomb of his friend.

> "And so, all the night-tide, I lie down by the side
> Of my darling—my darling—my life and my bride,
> In the sepulchre there by the sea—
> In her tomb by the sounding sea."

In the same poem his proud spirit defies even death, when he cries

> "And neither the angels in heaven above,
> Nor the demons down under the sea,
> Can ever dissever my soul from the soul
> Of the beautiful Annabel Lee:"

"The Philosophy of Composition" claims to explain the conception and composition of "The Raven," and while it is very doubtful if he really wrote the poem in any such impersonal, intellectual and rational manner, nevertheless his analysis contains some valuable hints, as when he writes: "Now, never losing sight of the object *supremeness* or perfection, at all points, I asked myself—'Of all melancholy topics, what according to the *universal* understanding of mankind, is the *most* melancholy?' Death—was the obvious reply. 'And when,' I said, 'is this most melancholy of topics most poetical?' From what I have already explained at some length, the answer here also is obvious—'When it most closely allies itself to *Beauty;* the death, then, of a beautiful woman is, unquestionably, the most poetical topic in the world—and equally is it beyond doubt that the lips best suited for such a topic are those of a bereaved lover.'" The psychoanalyst is prone to seek farther into the poet's life to explain his preoccupation with death. It will be recalled that his parents

died early and under most unhappy circumstances; many an hour the sensitive, imaginative child must have brooded over the tragic end of his beautiful, gifted mother. Then at the impressionable adolescent period came the death of the woman he worshipped, Mrs. Helen Stannard. A few years later died the foster mother. Three sweethearts were lost to him. Then there were the six years of dread for Virginia's life. These he has described in a letter to a friend, dated January 4, 1846 (7, p. 215): "Six years ago, a wife, whom I loved as no man ever loved before, ruptured a blood vessel in singing. Her life was despaired of. I took leave of her forever, and underwent all the agonies of her death. She recovered partially, and I again hoped. At the end of a year, the vessel broke again. I went through precisely the same scene. . . . Then again—again—and even once again, at varying intervals. Each time I felt all the agonies of her death—and at each accession of the disorder I loved her more dearly and clung to her life with more desperate pertinacity. But I am constitutionally sensitive—nervous in an unusual degree. I became insane, with long intervals of horrible sanity. During these fits of absolute unconsciousness, I drank—God only knows how often or how much. As a matter of course, my enemies referred the insanity to the drink, rather than the drink to the insanity. I had, indeed, nearly abandoned all hope of a permanent cure, when I found one in the *death* of my wife. This I can and do endure as becomes a man. It was the horrible, never-ending oscillation between hope and depair which I could *not* longer have endured, without total loss of reason. In the death of what was my life, then, I received a new but—Oh God!—how melancholy an existence." Allowing all due discounting for the poetic picturing of himself in constant agony, there can be no doubt that "the horrible, never-ending oscillation between hope and despair" of those six years must have made an indelible impression upon the poet's brain. Little wonder, then, that he wrote of the death of beautiful women.

In his stories Poe continues this linking of death with sex. Another factor manifests itself, namely, the sadistic delight in torture. Let us recall a few of these stories. In "Berenice" the lover tears out the teeth of his beloved before her body is cold within the grave; in "The Black Cat," having cut out the eyes of the cat he abhors, and later killed it, he is tormented by another cat whom his wife protects; maddened by her attitude he seizes an ax and cuts through her brain, after which he walls up the body in his cellar and laughs with glee at the fumbling search of the police; there is the murder

of Marie Roget, mutilation of the body and sinking of it in the river; in the "Murders in the Rue Morgue" he dwells over all the gruesome details of the masses of hair torn from the old woman's head, the throat cut with a razor, the daughter stifled in a chimney. The record was kept of seventeen stories of Poe as they appeared in an ordinary edition. Of these tales only three did not record violent or tragic deaths; these were "The Purloined Letter," which ends with a revenge; the "Man in the Crowd," a personification of crime, and the "Pit and the Pendulum," as terrible a delineation of torture as has ever been penned. Here is the death list from the other fourteen: Two dead in the "Gold Bug," two in "The Descent into the Maelstrom," the crews of two ships in the "Mss. Found in a Bottle," two in the "Murders in the Rue Morgue," two in the "Mystery of Marie Roget," two in the "Fall of the House of Usher," one in "William Wilson," one in "The Black Cat," one in "The Tell-Tale Heart," two in "The Assignation," the prince and his thousand attendants in "The Masque of the Red Death," one in "Berenice," two in "Morella," one in "Shadow." The list might be extended to cover the majority of the tales, but it seems unnecessary to pursue such inquiry further. Poe's mind was not only preoccupied with death but with violent death, with murder and with pestilence.

The sadistic impulse is readily linked with sex. This is shown in a more subtle form in his favorite story "Ligeia." Here is the description of the bridal chamber prepared for his second wife:

"Some few ottomans and golden candelabra, of Eastern figure. were in various stations about—and there was the couch, too, the bridal couch—of an Indian model, and low, and sculptured of solid ebony, with a pall-like canopy above. In each of the angles of the chamber stood on end a gigantic sarcophagus of black granite, from the tombs of the kings over against Luxor, with their aged limbs full of immemorial sculpture. But in the draping of the apartment lay alas the chief fantasy of all. The lofty walls, gigantic in height—even unproportionably so—were hung from summit to foot, in vast folds, with a heavy and massive-looking tapestry—tapestry of a material which was found alike as a carpet on the floor, as a covering for the bed, and as the gorgeous volutes of the curtains which partially shaded the window, the material was the richest cloths of gold. It was spotted all over, at irregular intervals, with arabesque figures, about a foot in diameter, and wrought upon the cloth in patterns of the most jetty black. But these figures partook of the true character of the arabesque only when regarded from a single point of view . . . To one entering the room, they bore the appearance of simple monstrosities; but upon a farther advance this appearance gradually departed; and step by step, as the visitor moved his station in the chamber, he saw himself surrounded by an endless succession of the

ghastly forms which belong to the superstition of the Norman, or arise in the guilty slumbers of the monk. The phantasmagoric effect was vastly heightened by the artificial introduction of a strong continual current of wind behind the draperies—giving a hideous and uneasy animation to the whole." Here again is the picture of the bed which is a coffin, and the heavy curtains move uneasily like the pall which covers the last resting place of the dead. In the center of the room swings the censer around which writhe serpent-like flames; the censer is of gold. In tending the sick lady Rowena the husband offers her a cup of wine, into which he sees fall from an invisible hand a few ruby colored drops. Insensibly the reader feels that this is the revenge of the first wife, Ligeia, on the one who has taken her place. Rowena dies, then revives, dies again, revives. Each time the husband does what he can to assist her, although he seems to desire that she shall die utterly and leave him in peace. When he thinks she is at last quiet in her final sleep she rises and reveals to him the wild eyes and streaming hair of his lost love Ligeia.

In this tale are the three colors: gold and black and red, which Poe uses most; there is also beauty and death and sex. The bed and the entire room suggest a coffin in a tomb, the censer which is so frequently present in his descriptions is reminiscent of the church services which he attended twice each Sunday in England and so may very easily have become associated with funerals and death, the serpent-like flames represent the sex symbol. Here, too, is found another characteristic of much of his writing: the death wish, this time directed against the second wife. Then comes again the old feeling that the dead are not wholly dead and the dead woman is revealed as his first wife. Her expression is the projection of his own feelings of remorse both for taking another bride and for the death-wish against Rowena.

The imagery of this story Poe uses over and over again. "The Assignation," another story of the love of a neurotic and the death of a beautiful woman, gives the picture of a strange room where "Rich draperies in every part of the room trembled to the vibration of low, melancholy music." Here also are the swinging censers; the windows are of crimson tinted glass. In the "Pit and the Pendulum" the condemned wretch says, "I saw, too, for a few moments of delirious horror, the soft and nearly imperceptible waving of the sable draperies which enwrapped the walls of the apartment." In "The Raven" is heard the "silken, sad, uncertain rustling of each purple curtain." "Metzengerstein" has tapestry hangings which swing gloomily upon the walls. In "Shadow" are sable draperies. We read in "The Masque of the Red Death:" "The seventh apartment was closely shrouded in black velvet tapestries that hung all over the ceiling and down the walls, falling in heavy folds upon a car-

pet of the same material and hue." It was not merely the thought of death which obsessed the poet, but death in all its details and in its visible symbol, the casket of the dead. From contemplation of the coffin the funeral pall or draperies came to have a peculiar significance, and when he wrote of rooms in which terrible deeds transpired he hung those rooms with the draperies of the dead. Not only that, but the curtains stir, to sad music, to strange winds, heightening the effect of horror, as the breezes which lift the pall over the face of the dead, increasing the agony of the spectator by giving the semblance of life where life has fled. In his poems are the same figures: a curtain is a funeral pall ("The Conqueror Worm"), "there passed, as a shroud, A fleecy cloud" ("Evening Star"),the eyes of the beloved "desolately fall on my funereal mind Like starlight on a pall." (To—.)

Then there are the colors: gold sometimes, but the principal contrast being the black and the red, symbols of death and of sex, the two ideas to which his thoughts ever returned. Black ships, black cats, clocks of ebony, sable condors, black marble flagstones, blackened goblets, black wings of pestilence, ebony tables, black silk-velvet palls, sable hearse plumes—these are a few of the symbolizations of death. For the sex motif are red lights, crimson-tinted glass, scarlet panes, the ruddy reflection from burning buildings, the fiery colored horse, fiery colored clouds, blood-red metal, intense light of rubies, the red of poppies, wine red as blood, rain that changed to blood, the fiery wall of the horizon, red clouds, the red eye of the sun, the crimson moon. No attempt has been made to cover all the uses Poe has made of the two colors, but it is very significant that these are the two which he used most often, and on which he rang so many changes. "The Masque of the Red Death" shows the effectively vivid combination of the two. The seventh chamber, completely covered with black draperies, has for windows scarlet panes, outside of which lamps are placed, so that the light falling on the black curtains must pass through the scarlet and so blend the two in a weird and unearthly aspect.

After the coffin, the poet's mind turned to the grave or tomb, and a vast number of allusions to this may be found in his writings. In "The Cask of Amontillado" he walls up his enemy in a living tomb; in "The Fall of the House of Usher" Madeleine is placed in a vault under the house; in "The Assignation" he rides in a "funereal gondola;" the "Gold Bug" is the story of treasure buried along with two men in a single grave; the Red Death masqueraded in grave clothes; he buries

his victim under the floor in "The Tell-Tale Heart;" he thinks "what sweet rest there must be in the grave" ("The Pit and the Pendulum") and in the same story "dreaded to move a step, lest I should be impeded by the walls of a *tomb*." He sings of the worm that never dies and prays that worms may creep softly around his sweetheart's body. In this connection also will be recalled his poem, "The Conqueror Worm."

The creeping of worms through dead bodies is considered a sexual symbol, and in this connection seems to show the presence of an erotic gratification at the thought of the dissolution of the bodies of beautiful women. The thought of the sweet rest within the grave is the desire to creep back into the mother's womb, which Jung and Silberer have considered so characteristic of the introvert.

The sadistic element is a compound of the sexual desire and the desire to give pain. These desires, repressed in Poe's life, are embodied in his poems and tales. With the child-wife, who was for years almost an invalid, he could never have enjoyed the satisfaction of a normal sex life, while his love for other women seems to have been of the mind rather than the body. Thwarted on this side of his nature, he turned to literary and artistic creation. In this he eked out the barest livelihood, and made enemies constantly by reason of his genius as well as of his intolerance of the claims of the many mediocre. Through the courts and by means of powerful invectives from his own brilliant, bitter pen he sought revenge on those who slandered or opposed him. But his victories were never wholly won; he never triumphed utterly over his enemies. His will to power, however, would be satisfied with nothing short of their annihilation, and, failing that in the actual physical world as well as in the literary and social world, he turned loose his sadistic impulses upon the creatures of his stories, some of whom must have represented very real figures to himself. In "The Cask of Amontillado" he says a wrong is never righted till the aggressor is punished and knows he is punished by the injured one. His imagination fairly gloats over the ingenious tortures it devises, many of which are equal to the most fiendish of the Inquisition.

Freud (4) has written on the effect which whipping has on children in developing both sadism and masochism. On seeing other children whipped the child feels that he is then the favored one, and a sadistic element of joy at suffering is introduced. On being whipped himself the child may be animated either by a sadistic or a masochistic desire. Masochism can be explained as a turning back of sadism upon the ego and is not itself a primary sex urge. It is a sort of narcissistic

or autoerotic sadism, or sadism focussed on the person's own genital organs. Poe went to school in England at a time when fagging was in order and as a little fellow he must have come in for his due amount of punishment. In school at Richmond, under a schoolmaster who believed greatly in the efficacy of flogging, Poe was never known to be punished, this fact being remarked on as quite unusual by one of his classmates. His experience in England would allow for development of either masochistic or sadistic elements; that in Richmond would reinforce the sadistic. The son of his foster father's partner remarked on the mean delight which Edgar, then a boy, took in tormenting a sensitive girl by pointing an imitation serpent towards her. (6, p. 25.) The serpent being the sexual symbol, the victim of the cruelty a girl, there seems an undoubted sadistic element here. The same gentleman tells of being thrown into some falls by Poe, who was then obliged to rescue the weaker boy, such an incident gratifying both the sadistic tendency and the will to power. The scene which he is reported to have made with the second Mrs. Allan when she was sick in bed indicates the same sadistic trend. All through his life the two things are found together: his will to power, thwarted, demanding sadistic revenge, his sadism gratifying and reinforcing his will to power.

There is an obvious death wish in many of the stories of Poe directed against a man. In "The Tell-Tale Heart" it is against an old man, whose murder he accomplishes; not satisfied with this he cuts off the limbs. He commences the story in an extenuating tone, but objects to being called mad. "True—nervous—very, very, dreadfully nervous I had been and am; but why *will* you say that I am mad?" Then telling of the murder, he continues: "It is impossible to say how first the idea entered my brain; but once conceived, it haunted me day and night. Object there was none. Passion there was none. I loved the old man. He had never wronged me. He had never given me insult. For his gold I had no desire. I think it was his eyes! yes, it was this!" This detailed setting forth of lack of reason for a murder indicates in the writer a defense mechanism against the unconscious wishes. There was one man for whose murder all such reasons would apply: the foster father, Mr. Allan. There had never been love or understanding between the two. He had brought up the gifted boy as his son, giving him his name, then had cast him off without a cent. He had insulted him and driven him from his house. He had refused to pay Edgar's gambling debts which the boy regarded as debts of honor, and so had placed

him in a position of galling humiliation at the University. Had he died earlier Edgar would have inherited his gold. Had he not married again the fortune would probably have gone to the poet. In all the years of destitution, of pitiful struggle against a pitiless fate, never able to realize his cherished desire of founding a magazine because of lack of funds, seeing a young wife fade away lacking the money to buy her either the drugs or food her condition demanded—is it not reasonable to suppose that hatred should turn against the man who might have prevented all this, against the man who had taught him to expect and to desire luxuries and had disappointed him, against the man who had humiliated and insulted him? Object and passion were surely here sufficient for the unconscious if not the conscious death wish.

We come now to the masochistic tendency of Poe, which while not nearly so striking generally, is extremely well displayed in a few stories. It will be recalled that masochism is the regression of the sadistic impulse from the outer world onto one's own person. In "Loss of Breath" he describes his own tortures in tones of considerable enjoyment, his ears were cut off, incisions made into his stomach, the skull fractured, he was hanged and obligingly went through spasms for the benefit of the populace which encored. "A Predicament" tells the story of his being caught in a huge clock and the minute hand being imbedded in his neck till it cuts off the head.

"The ticking of the machinery amused me. *Amused me*, I say, for my sensations now bordered upon perfect happiness. . . . The eternal click-clack, click-clack, click-clack, of the clock was the most melodious of music in my ears. . . The bar had buried itself two inches in my neck. I was aroused to a sense of exquisite pain . . . My eyes, from the cruel pressure of the machine, were absolutely starting from their sockets . . . one actually tumbled out of my head . . . I was presently relieved . . . by the dropping out of the other eye.

The bar was now four inches and a half deep in my neck, and there was only a little bit of skin to cut through. My sensations were those of entire happiness, for I felt that in a few minutes, at farthest, I should be relieved from my disagreeable situation . . . At twenty-five minutes past five in the afternoon precisely, the huge minute-hand had proceeded sufficiently far on its terrible revolution to sever the small remainder of my neck. I was not sorry to see the head which had occasioned me so much embarrassment at length make a final separation from my body. It first rolled down the side of the steeple, then lodged, for a few seconds, in the gutter, and then made its way, with a plunge, into the middle of the street."

The detail with which this is told and such expressions as "amused me," "a sense of exquisite pain," "entire happiness,"

are expressive of masochistic delight in suffering while the sardonic humor which laughs at his own dismemberment is a defense mechanism which the sensitive poet must have had to set up early in life, as protection against the ridicule of others.

"The Pit and the Pendulum" is the tale which best of all illustrates this masochistic tendency. It is told in the first person by a victim of the Inquisition. The man swoons on hearing his sentence and awakens to find himself in utter darkness in an underground cavern. There he narrowly escapes falling into a grewsome pool of nameless horrors. The invisible torturers plan for him one death after another, each more terrible than the last. In reality one feels that it is the author himself who takes delight in planning first, one affliction, then another, and who enjoys the thought of his own body suffering such agonies. The story is like a dream in which we watch ourselves take part, feel with ourselves and yet are detached from ourselves. After a swoon the poor victim awakens to find himself bound flat upon the floor, utterly helpless save for one free arm with which he can reach the scanty food at his side, which is carefully calculated just to keep life in his body. He hears above him a strange ticking and perceives in the shadows what appears to be a large pendulum. As the hours pass he sees this pendulum slowly descend. It approaches his bound body, hour by hour coming nearer; after every swoon he awakens to find it closer; he sees the sharp steel of its point; it cuts the air in ever wider circles; fascinated, yet horrified, he watches for days the approach of death; in detail the author describes his body sensations. The rats have come out to snatch the last remnants of food; at length it occurs to him that by smearing the meat upon his bonds they will gnaw away his fetters. While the rats gnaw at his cords the steel pendulum sweeps nearer and nearer, its sharp blade cutting the air just above him. It touches his garments, it cuts the cloth above his breast, and just at the last second he is freed and crawls away. Immediately the pendulum is drawn up, showing how incessantly he is watched. Then there comes a light within the chamber, a strange light which shows up the horrible, fantastic figures of devils and ghosts upon the walls; these figures begin to glow with heat, the walls and floor are hot to the touch, he perceives that they are entirely of brass and capable of casting out an intense heat. Tortured by fear and by the increasing heat, he creeps to the edge of the pool, and gazes down, too horrified by what he sees to take the plunge. It is evident that the torturers design to drive him into the pool and he determines

to resist at no matter how great suffering. The heat and his agony continue till at the limit of human endurance he is rescued by the armies of his sect who have taken the town. The meticulous detail of this story, the evident relish with which the author describes his sensations as, helpless on the floor he watches the nearing sweeps of the sharp blade of the pendulum, or as he recoils from the ever increasing heat, shows a masochistic delight in the thought of torture applied to his own body.

Mordell (9) calls attention to the stories of ratiocination of Poe which he considers a further indication of masochism. "The Murders in the Rue Morgue," "The Mystery of Marie Roget," "The Purloined Letter," and "The Gold Bug," are masterly examples of the analytical genius of Poe. The careful study of the tedious details of the mysteries of the first three as well as the solving of the cipher in the last tale represent the delight of a mind which loved to torture itself.

This is clearly shown in the man's life as well. As editor of a Philadelphia journal he made the boast that man could invent no cipher which human ingenuity could not decipher, and set himself the task of solving the many cryptograms submitted. The busy editor yet found time to spend hours exercising his mental faculties, as if delighted by the irritating burdens he could place upon his mind. It is reputed that he never failed to make good his boast.

The analytical powers manifested by Poe in such stories as those mentioned above, and also strikingly evident in much of his critical writing, was a characteristic not commonly associated with his other gifts. He insisted, however, that there could be no genuine imagination without this capacity for analysis of the keenest sort. Certainly the verisimilitude of his most extravagant tales is greatly increased by the background of incisive thinking and detailed analysis which they evidence. Oliver Leigh has made an interesting study in the character of Poe as shown by his photographs. One side of the head, if duplicated, gives a picture of the bulging forehead and heavy brain of the hydro-cephalic, the dwelling place of grotesque fancies and weird images of horror; the other side shows the square-headed, intellectual, analytical type, the brain which produced stories of ratiocination, critical essays of penetration and which distinguished itself in mathematics. (8) These two types, confined within one brain, gave the world the dreamer, the neurotic, the poet, the analyst and the thinker.

Poe's life after the death of Virginia in January, 1847, is sadder than any of his stories. The last two years before the

end in 1849 are a miserable record of alternating hope and despair, of a body and mind on the downward grade of deterioration, checked now and again before its approaching dissolution to turn and attempt to struggle back to sanity and health, yet never quite succeeding. After his own illness at the time of his wife's death he never regained his former capacity of productivity, although his last years were marked by fitful gleams of that genius which has won for him in Europe the reputation of being America's one really great and original poet. The mad poem, "Ulalume," with its melody and weird effects of nameless horror; the beautiful death poem, "To Annie," in which he thanks Heaven that "the fever called living is over at last;" the onomatopoeism of "The Bells" and the prose poem, "Eureka," belong to this period. In these last years he also met and loved Mrs. Helen Whitman, formed an ardent friendship for "Annie," Mrs. Richmond, and became engaged to Mrs. Shelton who, as Miss Royster, had been his boyhood sweetheart. The last thing he wrote was the lyrical "Annabel Lee," which was not published until after his death.

"Eureka" represents a new departure into philosophical realms. Dedicated to Alexander von Humboldt it is offered to "the dreamers and those who put faith in dreams as in the only realities" as a "Book of Truths, not in its character of Truth-Teller, but for the Beauty that abounds in its Truth; constituting it true." Through all its excursions into Physics, Astronomy and higher mathematics it manifests the neurotic passion for Unity—a seeking after a primal cause from which came all the Universe and to which all must return. In this are passages suggestive of Ward's "primal homogeneity" and theory of filiation. The poet anticipates the time when the myriads of individual intelligences will become blended, as will the bright stars, into One. He closes with the triumphant thought: "Think that the sense of individual identity will be gradually merged in the general consciousness—that Man, for example, ceasing imperceptibly to feel himself Man, will at length attain that awfully triumphant epoch when he shall recognize his existence as that of Jehovah. In the meantime bear in mind that all is Life—Life—Life within Life—the less within the greater, and all within the Spirit Divine." (10c, p. 315.)

The rumors which still go the round of the clubs in Baltimore claim that Poe was definitely syphilitic. This has never been established, although the lesion on the brain and cerebral congestion from which he suffered in his last years increase

the probability of the syphilitic infection. Whether this be true or not, he certainly displayed the same diathesis evidenced by many men of genius who are said to have been syphilitic—for instance, de Maupassant, Wilde, Nietsche. In them, as in Poe, the phyletic tendencies died out and the egoistic ones came into dominance. This might be expected as a result of the disease which, depriving a man of the capacity for propagation, so cuts him off from the normal social interest in the future of the race, and turns the full force of the libido in the direction of personal ambition. Senescents and young people—those too young and too old to bear children—betray somewhat this same selfish absorption in the attainment of their own desires. These syphilitic geniuses, driven by the nature of their disease, to concentration upon their own egoistic development as the sole means of satisfying their will to power, became to a great extent hyper-individualized, with an over-expansion of the ego which made them anti-social. Their sex interests, too, seem to make at least a partial transference from the normal object of the opposite sex. Oscar Wilde's glittering descriptions—of pomegranates bursting in the sun, of tall reeds of fluted ivory, of pale poppies, ostrich plumes like white foam, robes of tissued gold, ceiling of fretted silver, green bronzes, agates, lapislazuli, bowls of amethyst, nightingales, and faint perfumes of jasmine,—the manner in which he lingers lovingly over long pages of rare gems, rich embroideries and draperies, strange, heavy odors, and the sound of weird, barbaric music, betrays more than a mere esthetic appreciation of beauty. There is a sensuous, sexual delight in these beautiful objects which far surpasses the normal enjoyment. Poe manifests this same attitude, although his range of enjoyment is more narrowed than that of Wilde, and even in his keenest appreciation of the loveliness of the inanimate world he is still haunted by the conceptions of horror and of death, which were to him inseparable from the realization of the highest beauty. This, too, would indicate a certain sexual displacement because of his constantly allying sex with death—the death of a beautiful woman being to him the most poetic of themes. Likewise his love of women, which was apparently of the mind so much more than of the senses, seems to signify an erethic sublimation which may have been, in the last analysis, based upon his physical condition.

It was in these last two years that Poe met and became engaged to Mrs. Helen Whitman. His love letters to "Helen" as well as his tenderly devoted letters to "Annie" at this time,

form some of the finest products of his later writing. These letters to "Helen" have been extravagantly praised as showing the passion of a lover. This they do not. They show the passion of a poet, writing about love. He was writing literature under the form of love letters just as he had under the form of love poems, stories or critiques. This passage has been often referred to as showing his deep devotion to Mrs. Whitman: (11, p. 71)

"As you entered the room, pale, timid, hesitating, and evidently oppressed at heart; as your eyes rested appealingly, for one brief moment, upon mine, I felt, for the first time in my life, and tremblingly acknowledged, the existence of spiritual influences altogether out of the reach of my reason. I saw that you were Helen—*my* Helen—the Helen of a thousand dreams—she whose visionary lips had so often lingered upon my own in the divine trance of passion—she whom the great Giver of all Good preordained to be mine—mine only—if not now, alas, then at least hereafter and *forever* in the Heavens. You spoke falteringly and seemed scarcely conscious of what you said. I heard no words—only the soft voice, more familiar to me than my own, and more melodious than the songs of the angels. Your hand rested in mine, and my whole soul shook with a tremulous ecstasy. And then but for very shame, but for fear of grieving or oppressing you—I would have fallen at your feet in as pure—in as real a worship as was ever offered to idol or to God. And when, afterwards, on those two successive evenings of all—heavenly delights, you passed to and fro about the room—now sitting by my side, now far away, now standing with your hand resting on the back of my chair, while the preternatural thrill of your touch vibrated even through the senseless wood into my heart—while you moved thus restlessly about the room—as if a deep Sorrow or more profound Joy haunted your bosom—my brain reeled beneath the intoxicating spell of your presence (and it was with no human senses that I either saw or heard you. It was my soul only that distinguished you there.) I grew faint with the luxury of your voice and blind with the voluptuous lustre of your eyes."

Yet at the same time that he was writing to Mrs. Whitman in such a vein, he was writing to "Annie" in letters breathing a spirit of entire confidence and love. Mrs. Whitman was said to have been greatly hurt by the publication of these latter letters, but they in no sense diminish Poe's love for her, nor show any evidence of faithlessness. In all these letters he was simply being true to himself—true to his own needs which required the loving approval of many women rather than the utter devotion of one, which could be satisfied as much by the sexless attachment to a married woman as by his engagement to Helen. After the breaking of the engagement with Mrs. Whitman, brought about by the efforts of friends of hers who objected to the match, Poe became engaged to Mrs. Shelton. His relations with women after Virginia's death represent a pitiful attempt to re-establish himself, to regain

from the eyes of those who loved him his own self-respect. Mrs. Clemm was then too old and broken to afford him the solace and safety he so hopelessly craved. But from among all his women friends, from all who had relieved the dark hours of Virginia's death, all those who from their attachment to the poet, ministered willingly to his mother-in-law after his death, no Gradiva appeared, not one was strong enough to rescue him from the perilous path his feet were straying on, not one was able to bring him peace, and faith and a renewed interest in life.

But more than a woman's love he was needing the attention of a specialist and this he did not receive. The libido of the poet, thwarted in its former expression through his poems and his love for his wife, sought here and there for other outlets, in his attachments to other women, in his titanic dream of the universe wherein he sought to grasp the very essence and simplicity of Unity itself, and in his obsessive desire to found a magazine of his own,—sought, but did not find. The lesion on one side of his brain (diagnosed by the famous New York physician, Dr. Mott, and the nurse, Mrs. Shew, Poe's faithful friend for years) would not permit him to use stimulants or tonics without producing total insanity. N. P. Willis, the editor, in a tribute to Poe immediately after the unhappy end in Baltimore, alludes to this: "We heard, from one who knew him well, . . . that, with a single glass of wine, his whole nature was reversed, the demon became uppermost, and, though none of the usual signs of intoxication were visible, his *will* was palpably insane." (6, p. 361.) Yet in times of deepest depression even insanity must have come to appear preferable to his own sad thoughts, and he sought feverishly "surcease from sorrow," whatever the means or the result. Before going to Richmond on the final journey he wrote to Annie: "No, my sadness is *unaccountable,* and this makes me the more sad. I am full of dark forebodings. *Nothing* cheers or comforts me. My life seems wasted—the future looks a dreary blank: but I will struggle on and 'hope against hope.'" (6, p. 303)

The attempted suicide from laudanum in Boston was only one evidence of the profound state of melancholia into which he had sunk. His physique deteriorated, and suffering from congestion of the brain, he should have been in a sanitarium instead of struggling about the country in the forlorn hope of founding his magazine. It was in quest of this "will o' the wisp" that in 1849 he set out for Richmond. He was at this time in the most wretched spirits; he expressed a pre-

sentiment to Mrs. Lewis that he would not return and arranged all his papers with Mrs. Clemm, telling her what to do in case he died. His delusion of persecution in Philadelphia is extremely suggestive of the beginnings of paresis. His state is described by Mr. Gill (6, p. 307):

"When he finally reached the residence of his kind friend, Poe was in a highly excited condition, almost distracted indeed. His mind seemed bewildered and oppressed with the dread of some fearful conspiracy against his life; nor could the arguments or entreaties of his friend convince him that some deadly foe was not, at that very moment, in pursuit of him. He begged for a razor for the purpose of removing the mustache from his lip, in order, as he suggested, that he might disguise his appearance, and thus baffle his pursuers. But, unwilling to place such an instrument in his hands, he was prevailed upon to allow his host to effect the desired change upon which he imagined his safety depended. The conditions of Poe's mind was such that Mr. Sartain, after persuading him to lie down, remained watching with him through the night with anxious solicitude, unwilling to lose sight of the unfortunate sufferer for a moment. The following night, Poe insisted on going out. He turned his steps towards the River Schuylkill, accompanied, however, by his devoted friend, whose apprehension was strengthened by the vehemence with which, without cessation, he poured forth in the rich, musical tones for which he was distinguished, the fervid imageries of his brilliant but over-excited imagination. The all-absorbing theme which still retained possession of his mind, was the fearful conspiracy that threatened his destruction. Vainly his friend endeavored to reassure and persuade him. He rushed on with unwearied steps, threading different streets, his companion striving to lead him homeward, but still in vain.

"Towards midnight, they reached Fairmount and ascended the steps leading to the summit, Poe all the while giving free scope to the conversational powers for which he was always remarkable, insisting upon the imminence of his peril, and pleading with touching eloquence for protection. . . .

"He didn't recover from this intense excitement until, subsequently, escaping from the house, he wandered out into the neighborhood of the city, and throwing himself down in the open air in a pleasant field, his shattered nerves found a comfortless but sorely needed repose. He woke refreshed. . . .

"All that he could call to mind were the entreaties and persuasions of some 'guardian angel' who had sought to dissuade him from a frightful purpose."

Such delusions would seem to have some correlation with the stories that he was unable to go to sleep after Virginia's death without the presence of Mrs. Clemm or some kind friend at his bedside. The fear of death, which he evidenced in his morbid prepossessions with the subject, may have grown in these "lonesome latter years" to be so portentous and awful a figure that he could never quite escape its shadowy horrors. Or again, the fear of life itself, of the futile mockery of life which he was leading, of himself as he had come

to be in his weakness, might have been the cause of much of his terror. Either fear could easily, in his overwrought state, have led to the thought of suicide, which he had already once attempted, and this may have been the "frightful purpose" from which the "guardian angel" sought to dissuade him.

Temptation conquered for the time being, there came to him a momentary peace from his delusions and fears and a new enthusiasm for laying hold on the actual facts of reality. The possibility of a new life beckoned him smilingly, and he arrived in Richmond in unusually good spirits. There he was welcomed and made much of by old friends. He even took the temperance pledge with much earnestness, obtained a position in the city of his childhood and planned to bring Mrs. Clemm down there. It was at this time that he is said to have renewed his addresses to Mrs. Shelton and she, a widow of independent means, to have accepted them. He lectured before a cultured audience, and left Richmond apparently more contented than he had been for some time, carrying with him, according to Bishop Fitzgerald (6, p. 322), the proceeds of his lecture, which amounted to $1,500. Of the tragic, solitary end in Baltimore, little is known with any degree of accuracy. Poe arrived there at election time, at a period when it was customary for bands of men to seize helpless strangers, drug them and carry them from poll to poll, registering their votes for whatever party they desired. It is believed generally that he fell in with such a gang. However that may be, he was found in a barroom, insensible, in the shabbiest of clothes. He was taken to a hospital where he lived about a week. He is reported as being part of the time in stupor, part of the time in delirium, during which he conversed vacantly with spectral or imaginary objects. His replies to questions were entirely unsatisfactory. Two of his doctors report that he made the statement that his best friend would be he who would blow out his brains. (6, pp. 334-336.) So, on October 7, 1849, was extinguished that flame of genius which had blazed, now so fitfully, now so brilliantly, for forty years. In the mystery of his last days, his life did indeed go out like a candle in the dark.

It is doubtful if the circumstances of his death matter so very much. If Baltimore rowdies came in to hasten the end, they were simply an incident in the inevitable close. The man had burnt himself out. He had described his own condition when he wrote: "There are few men of that peculiar sensibility which is at the root of genius, who, in early youth, have not expended much of their mental energy in *living too fast;*

and, in later years, comes the unconquerable desire to goad the imagination up to that point which it would have attained in an ordinary, normal, or well-regulated life. The earnest longing for artificial excitement, which, unhappily, has characterized too many eminent men, may thus be regarded as a psycal want, or necessity—an effort to regain the lost—a struggle of the soul to assume the position which, under other circumstances, would have been its due." (6, p. 231.) He showed at the last very clearly that "psychal" want, the struggle of the soul to regain the lost, and in the nature of the case he was doomed to failure. That last brief lifting of the clouds in Richmond could have been only temporary; his weakened body and the congestion of the brain from which he was suffering made inevitable the return of his former condition of melancholia. That diseased brain which had supplied his writings with all its morbid, beautiful imagery and haunting melodies of death, came finally by its defects to render his further progress and development impossible. And when it failed him, and there was no longer possible a continuation of the superiority which he had won in his chosen field, his proud spirit, his guiding fiction, must have sought relief in some form of insanity or in death. Insanity had begun, under the depression of melancholia and the delusions of paresis; death he had sought before and would have sought again; the darkened close in Baltimore came as a release—a release not to be regretted by any who love Beauty as an end in itself and who crave for its expression a form no less perfect than that which Poe was able to give in the days of his greatest power.

BIBLIOGRAPHY

1. Adler, Alfred. a. The Neurotic Constitution (trans. by Glueck) Moffat Yard & Co., N. Y., 1917.
   ———, ———. b. Study of Organ Inferiority and its Psychical Compensation (trans. by Jelliffe.) The Nervous and Mental Disease Publishing Co., N. Y., 1917
2. Brill, A. A. Psychanalysis. W. B. Saunders Co., Phil., 1913.
3. Bohannon, E. W. The Only Child in a Family. *Ped. Sem.*, April 1898, pp. 475-496.
4. Freud, S. Ein kind wird geschlagen. *Internat. Zeit. f. artzliche Psychoanalyse,* Heftes III, 1919.
5. Gates, L. E. Studies and Appreciations. The Macmillan Co. N. Y., 1900.
6. Harrison, J. A. The Life of Edgar Allan Poe. Thos. Y. Crowell & Co., N. Y. 1902
7. Ingram, John H. Edgar Allan Poe; His Life, Letters and Opinions. Hogg, London, 1880, 2 vols.
8. Leigh, Oliver. Edgar Allan Poe, the Man: the Master: the Martyr. The Frank M. Morris Co., 1906.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDWIN G. BORING

XXIV. HIGHEST AUDIBLE TONES FROM STEEL CYLINDERS

By C. C. PRATT

The Psychological Laboratory of Clark University has a set of twenty-two steel cylinders designed for the determination of the upper limit of hearing. The set is similar to Koenig's and is manufactured by the Standard Scientific Company of New York. The present list price is $75. The cylinders are computed by the manufacturers on the basis of a calibration made for a greater length of bar to give the frequencies for three octaves of the natural diatonic scale from $c^5 = 4096$ to $c^8 = 32{,}768$ vs. inclusive. Each cylinder is slightly grooved at its two nodal points and is suspended horizontally by loops of fine tough cord which pass about these grooves. A brass hammer is used to secure vibration.

We have attempted unsuccessfully to calibrate these cylinders by means of the Kundt dust method.[1] Schwendt has reported his experiments with this method in determining the frequencies of the Koenig high forks and cylinders.[2] The forks afforded him little difficulty, but only after many trials was he able to secure the Kundt dust figures from the cylinders. We proceeded in the same manner with lycopodium powder distributed in thin glass tubes ranging from 20 to 8 mm. in diameter, but in only two instances did we get the dust to fall in figures which could be considered reliable for the determination of wave-length. With the cylinders d^5 and f^5 the lycopodium fell into periodic loops 3.83 and 3.27 cm. in length respectively. These values divided into the speed of sound at the given temperature give frequencies of 4511 and 5361 as against 4608 and 5461.33 calculated for these particular cylinders by the makers. Koenig[3] was inclined to believe that calculation gave values slightly too high; thus the importance of calibration becomes apparent if the cylinders are to be used for accurate work. It would seem, however, that the Kundt dust method is not very suitable for use with the cylinders. We spent two weeks of vacation-time in fruitless effort to get results, and the only report of the success of this method with the cylinders is Schwendt's brief mention. On the other hand, since calculation and calibration agree here to within 2%, we may perhaps assume that the cylinders as calculated and marked by their manufacturers are accurate for clinical work and class demonstrations, even though calibration will be necessary in research work.

We have used the cylinders at Clark University to determine the

[1] For a discussion of this method as applied to the calibration of high acoustic frequencies, see E. B. Titchener, *Experimental Psychology,* vol. 2, part 2, pp. 32ff.

[2] A. Schwendt, Experimentelle Bestimmungen der Wellenlänge und Schwingungszahl höchster hörbarer Töne, *Pflüger's Archiv,* 75, 1899, p. 356.

[3] R. Koenig, Ueber die höchsten hörbaren und unhörbaren Töne, *Wied. Ann.*, N. F. 69, 1899, p. 723.

tonal TR of sixteen observers, all of whom had had experience in psychological observation. Each observer was seated comfortably in a chair with his back to the cylinders at a distance of four feet, and was given the following instructions: "When the stimulus is presented, you are to report 'yes' if you hear a tone, and 'no' if you hear no tone. Try to report immediately. If you are doubtful of your judgment, say so." Preliminary practice was given to train O and to enable E to determine the critical stimulus. The two cylinders immediately on either side of the critical stimulus were used to complete a series of five stimuli, on each of which 100 judgments were taken. The stimuli were presented in haphazard order in series of 50 judgments with a brief pause between series. No observer experienced more than a temporary initial difficulty in recognizing the presence or absence of tone, and doubtful judgments were rare. They were permitted but not recorded. The whole experiment required about an hour. The accompanying table shows the percentage of affirmative judgments on each of the five stimuli for each observer.

TABLE

Relative frequencies of the judgment "Yes," meaning heard-tonal-quality, for struck steel cylinders of the frequencies indicated. Sixteen observers, one session each. One hundred judgments on each of five stimuli. The frequencies are calculated and not calibrated; see text.

Obs.	Calculated Frequency and Musical Note									
	10922.67	12288.	13653.33	15360.	16384.	18432.	20480.	21845.33	24576.	27306.67
	f^6	g^6	a^6	b^6	c^7	d^7	e^7	f^7	g^7	a^7
1	100	100	92	15	1					
2			100	100	58	9	0			
3			100	100	88	14	0			
4			100	100	87	8	0			
5				100	100	8	0	0		
6				100	100	9	0	0		
7				100	100	78	8	0		
8				100	100	67	0	0		
9				100	100	76	0	0		
10				100	100	84	0	0		
11				100	100	85	5	0		
12				100	100	100	0	0		
13				100	100	95	11	0		
14					100	100	29	0	0	
15					100	100	83	6	0	
16						100	100	81	0	0
Average	100	100	99.5	94.7	90.6	58.3	13.9	5.4	0	0

1. The table shows that differences between individuals are much greater than the variations of the single individual from judgment to judgment. Although the d of about 18,000 is most often the critical stimulus there were cases in which the e and f above and the c, b and a below were critical. The instrument, therefore, provides a means for the determination of individual differences, though the number of stimuli is too few to render the calculation of a limen possible, and though the frequencies as calculated are inaccurate within a small per cent.

2. It is extremely probable that under conditions of greater practice not more than one cylinder would turn out to be critical, that all cylinders of lesser frequency would give 100% affirmative judgments and all cylinders of greater frequency no affirmative judgments. Eight of our sixteen observers give this result even under our conditions of little practice. It is also probable that greater practice would produce more often the results given by observer 12, for whom no critical stimulus was present in the series. He always heard tone for b, c, and d and never tone for e and f. He was, it must be added, a more practiced observer than many of the others, and tended for this reason to reject doubtful judgments.

3. In general the cylinders indicate that the limen is less than 20,000 vibrations. The limens for cylinders cited by Titchener[4] are 20,000 vs. Stumpf mentions the "fast allgemein angenommene Tongrenze von 20,000 Schwingungen."[5] If we take the averages in our table and compute a limen by Urban's method (phi-gamma hypothesis) we get a value of 18,610 vs. The limen is also dependent upon intensity, which is seldom controlled in experiments and almost never calibrated. Nevertheless it would seem that the recent theories of vowel sounds that require a c of 32,000 vs. must be failing to take account of the facts of the limen.

4. Observer 1, who has the lowest limen, is older than any of the other observers. There is, however, no other evidence of age differences. Observer 1 is between 40 and 50 years of age; observers 2, 6, and 12 are between 30 and 40; all the other observers are between 20 and 30. It happens that the three observers with the highest limens are the most musically trained.

5. One observer, in characterizing the quality of the high tones, described it as a "singing" sound and likened it to a hiss. This observation suggests that even in instruments so little vocal as struck steel cylinders vowel-quality may be present. It is possible, however, that the "singing" sound arises from the beating of the two adjacent tones that Koenig thinks these cylinders produce.[6]

6. Our work contrasts strikingly with work by the Galton whistle in that doubtful judgments with the cylinders are unusual. The scarcity of doubtful judgments is, of course, to be accounted for in part by the use of stimuli so far above and below the limen, but it also appears that doubtful judgments are exceptional even for the critical stimulus. If this conclusion is borne out in further investigation, it will be desirable to use cylinders for liminal determinations instead of the whistle. The psychologist would need to have made up, however, a set of at least thirty cylinders lying within the octave $g^6 = 12288$ to $g^7 = 24576$.

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[4] *Op. cit.*, 41.

[5] *Bericht über den VI Kongress für experimentelle Psychologie*, 1914, p. 327.

[6] Koenig, *op. cit.*, p. 722.

[7] Titchener, *op. cit.*, p. 40.

BOOK REVIEWS

Psychologie und Verkehrswesen. By HANS A. MARTENS. (*Heft* 10 of *Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens.*) Edited by OTTO LIPMANN and WILLIAM STERN, 1919. 14 p.

The author discusses first the general value of psychological principles in the training and selection of traffic personnel, in the certification of professional chauffeurs, and the licensing of private automobilists. He then takes up, in survey fashion, the psychological factors of the signal system. He enumerates the psychological requirements of good signals and states the problems involved in learning and reacting to them and in avoiding accidents and especially collision with pedestrians. His main object is to show that like all other phases of economic life, transportation and traffic need the co-operation of consulting psychological experts in solving all those problems which involve the human element as an important constituent. L. R. G.

Die Psychische Eignung der Funkentelegraphisten. By OTTO LIPMANN. (*Heft* 9 of the *Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens.*) Edited by OTTO LIPPMANN and WILLIAM STERN, 1919. 40 p.

The first part of this article deals with the analysis of the mental functions of wireless telegraphers. Both receiving and sending wireless messages require good memory and ability to learn quickly. In sending messages, accuracy, speed, and certainty in movements are essential, but the sender is usually not limited in speed nor exposed to distractions, which two factors make the receiving a much more difficult task. Since the signals to be received and recorded are faint tones from 500 to 1,000 vibrations, the listener must be able to differentiate accurately both pitch and intensive differences; he must have a good memory for pitch in order to identify thereby the stations communicating with him; and he must be able to recognize different rhythms of sound signals and the changes they undergo when their speed is altered. He will be the more efficient the less he needs to translate these sounds into visual or motor terms. Subjective types of receivers who interpret what they hear are less valuable, at least from the militaristic point of view, than the objective type who merely writes down what he hears. The process of recording involves a division of attention between hearing the present sound and simultaneous writing what had just been heard. Finally, the receiver must be able to disregard a large number of distracting and conflicting noises from other stations and confine his attention exclusively to the particular sender with whom he is trying to communicate. These auditory disturbances offer the greatest difficulty to the beginner in wireless telegraphy and prove sometimes too much for him.

In the second part the author describes a complex method of testing beginners in wireless telegraphy for the purpose of determining their fitness for such work. The five letters, k, m, r, s, and v, of the Morse

alphabet were employed as stimuli, at a constant pitch of 1,000 vs., presented automatically by a special sending apparatus which transmitted the sounds through a number of individual telephone receivers to a group of learners. The stimuli were given in successive series in different orders, at four different degrees of intensity, and at seven different rates. In some of the series disturbing secondary sounds of 600, 700, and 900 vs. were introduced. Each observer wrote down what letters he heard, as nearly as possible in the order in which he heard them, and he was scored on the basis of the number of correct replies.

The last part gives the results of these experiments with seventy-one observers and describes also the methods of computing the data from various points of view, in order to take account of practice, fatigue, and distractibility. The index of efficiency for a given observer is based upon the combined results of all the series, which had previously been weighed. Two methods of weighing the series are discussed and compared, and a positive correlation coefficient of .8264 with a P. E. of .02656 is found for them. Unfortunately the experimental results from these 71 observers could not be compared with the outcome of the training which they underwent or with their success in actual service, although another investigator, who has also used this method as part of a much more extensive study, is reported to have had good results with it. L. R. G.

BOOK NOTES

Taboo and Genetics. By M. M. KNIGHT, IVA L. PETERS, and PHYLLIS BLANCHARD. N. Y., Moffat, Yard and Co., 1920. 301 p.

This book is an attempt to help the many who are now thinking much about sex relations and family. It is frank and in simple language and is sociological. The three authors have divided the field between them as follows: Dr. Knight writes the chapters on The New Biology and The Sex Problem in Society; Dr. Peters, those on The Institutionalized Sex Taboo, and Dr. Blanchard on The Sex Problem in the Light of Modern Psychology.

A General Introduction to Psychoanalysis. By SIGMUND FREUD. (With a Preface by G. STANLEY HALL.) N. Y., Boni & Liveright, 1920. 406 p.

The substance of this volume was evidently given in popular lectures to a non-medical audience, and its twenty-seven chapters cover pretty completely the entire field from the standpoint of Freud. After three introductory chapters the next eleven are devoted to phases of The Dream, and the remainder to the General Theory of the Neuroses. All those who want the latest authentic statement of what psychoanalysis is and means, and to know the attitude which Freud takes toward his critics, will welcome this book.

The Psychology of Functional Neuroses. By H. L. HOLLINGWORTH. N. Y., D. Appleton and Co., 1920. 259 p.

The author represented psychology in the U. S. A. General Hospital at the Plattsburg Barracks, N. Y., and saw in all nearly 1,200 cases, most of which were examined, some merely enough to test mental age while several hours each were spent on other cases. Midway in the author's stay came the armistice, the effects of which were full of psychological interest. No attempt seems to have been made to analyze intensively any case, but general ideas of this group of cases were chiefly kept in mind. There are almost no references to the really copious recent literature on the subjects here treated. Perhaps the author's most interesting results are those derived from his 116 questions which he asked his patients to answer. He takes an essentially negative attitude toward Freudianism. Desirable as it is that the psychological standpoint should be exploited to the very uttermost in the study of psychoneurotic cases, it is very essential that the author should have no psychological bias or at least should not display it.

Man's Unconscious Passion. By WILFRID LAY. N. Y., Dodd, Mead and Co., 1920. 246 p.

This book deals with The Total Situation, especially Pleasure-Pain vs. Reality; Conscious and Unconscious Passion; while chapter III distinguishes at great length between affection and passion. Then follow chapters on Insight, The Transfer of Passion, The Emotion Age, etc. The author certainly writes with more abandon here than in his two earlier books.

The Group Mind. By WILLIAM MCDOUGALL. N. Y., G. P. Putnam's Sons, 1920. 418 p.

This is "a sketch of the principles of collective psychology with some attempt to apply them to the interpretation of national life and character." The work falls into three parts: I, General Principles of Collective Psychology; II, The National Mind and Character; and III, The Development of National Mind and Character. A book of this ambitious sort will just now challenge attention as never before, but to the mind of the present writer it adds very little to our knowledge and its value will depend upon whether the author's highly schematic mind working in this complex field will succeed in having its *Leitmotive* accepted.

Mind and Energy. By HENRI BERGSON (tr. by H. WILDON CARR). N. Y. Henry Holt and Co., 1920. 262 p.

The publisher boldly tells us on the cover of this work that it "is the most important philosophical volume which has appeared since the same author's world-famous 'Creative Evolution.' It gathers together the threads of 'Matter and Memory,' 'Creative Evolution,' and 'Time and Free Will,' and with additional arguments and undiminished power drives home the conviction of the freedom of the will and the ascendancy of soul over matter." The book consists of lectures delivered at various times and places, with little and certainly no successful effort to bring them into unity.

Ethics and Natural Law. By GEORGE LANSING RAYMOND. N. Y., G. P. Putnam's Sons, 1920. 345 p.

This is a reconstructive review of moral philosophy applied to the rational art of living. This very voluminous author always writes well, and if perhaps too copiously compensates by giving excellent analyses of his chapters.

Human Traits and Their Social Significance. By IRWIN EDMAN. Bost., Houghton, Mifflin Co., 1920. 467 p.

This book was written for use in a course entitled "Introduction to Contemporary Civilization" for Freshmen and gives a bird's-eye view of the processes of human nature from man's simple inborn impulses on to their fulfillment in religion, art, science, and morals. The first part analyzes types of behavior; and the second, the four great activities of the human mind and imagination—religion, art, science, and morals.

Proceedings of the International Conference of Women Physicians. N. Y., Y. W. C. A. Press, 1920.

The proceedings of this most important conference are published in six volumes on as many topics which engaged the attention of the conference in 1919, viz.: General Problems of Health, Industrial Health, The Health of the Child, Moral Codes and Personality, Adaptation of the Individual to Life, Conservation of Health of Women in Marriage. The calling of this conference just after the

war to consider these fundamental questions seemed to the writer almost an inspiration, and if all the expectations formed were not realized it should not be ascribed to the fault either of the management or delegates but rather to the vast range and fundamental character of the topics treated.

Industrial Psychology. By BERNARD MUSCIO. N. Y., Dutton, 1920. 300 p.

These lectures were designed for general audiences and were given to workmen in 1916. The author first describes the aim of industrial psychology, then discusses the mental factors relevant to industry, selection of workers on the basis of natural fitness, the best method of work, and closes with a chapter on the desirability of applying psychology to industry.

Activism. By HENRY LANE ENO. Princeton U. Press, 1920. 208 p.

The author of this book is research associate in psychology at Princeton. He here discusses activity as an underlying hypothesis in this field, its planes and their interrelation, and units; consciousness, the meta-psychic plane, and activism and the historic problems of philosophy.

The Child Vision. By DOROTHY TUDOR OWEN. Lond., Longmans, Green and Co., 1920. 180 p.

This is a study in mental development and expression and is devoted especially to the art of composition. It is, on the whole, said to develop a method which seems to be interesting and valuable.

God's Smile. By JULIUS MAGNUSSEN. N. Y., D. Appleton & Co., 1920. 185 p.

This writer takes an unconscionable length of time, with many irrelevant details, to tell us of a very remarkable experience which befell him, a literary man and journalist, the results of which made him more or less of a medium and a believer in spiritism. The conviction that he had really received a letter from his dead father seems to have changed his life. A number of these letters are printed.

Famous Psychic Stories. Ed. by J. W. MCSPADDEN. N. Y., Thos Y. Crowell Co., 1920. 305 p.

Here are twelve tales of very different nature thought by the editor to be psychological. They are by Hawthorne, Poe, Collins, Oliphant, M. R. James, Benson, etc.

Success Fundamentals. By ORISON SWETT MARDEN. N. Y., Thos. Y. Crowell Co., 1920. 307 p.

These are uplift talks on health and success, efficiency, how to find yourself, confidence, the law of opulence, and salesmanship.

Concentration. By CHRISTIAN D. LARSON. N. Y., Thos. Y. Crowell Co., 1920. 94 p.

Intelligence and Social Valuation: A Practical Method for the Diagnosis of Mental Deficiency and Other Forms of Social Inefficiency. By RICHARD A. BEERY and S. D. PORTEUS. Publ. by The Training School, Vineland, N. J., 1920. 100 p.

Leib und Seele. By G. SOMMER. Leipzig, Teubner, 1920. 128 p.

Einführung in die Psychologie. By ALEXANDER PFÄNDER. Leipzig, Barth, 1920. 382 p.

Das Problem der Gültigkeit in der Philosophie David Humes: Ein Kritischer Beitrag zur Geschichte der Erkenntnistheorie. By HEINRICH HASSE. Munich, Reinhardt, 1920. 193 p.

Die Raumwahrnehmung der Blinden. By WILHELM STEINBERG. Munich, Reinhardt, 1920. 151 p.

Theorie der Empfindungsstärke und insbesondere des Weberschen Gesetzes. (*Heft* 3 of *Schriften zur Anpassungstheorie des Empfindungsorganes.*) By JULIUS PIKLER. Leipzig, Barth, 1920. 26 p.

Das Denken und die Phantasie. By RICHARD M. FREIENFELS. Leipzig, Barth, 1916. 341 p.

Pengard Awake. By RALPH STRAUS. N. Y., D. Appleton and Co., 1920. 299 p.

INDEX OF AUTHORS

(The names of authors of original contributions are printed in SMALL CAPITALS.)

INDEX OF SUBJECTS

(The subject headings of all contributions except Book Reviews and Notes are printed in SMALL CAPITALS.)